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Nederlandstalige samenvatting

De fractie huiseigenaars is sinds het einde van de Tweede Wereldoorlog sterk gestegen in de meeste Westerse landen, waaronder België. De volkstelling van 1947 rapporteerde dat 38,9% van de huishoudens de woning bezit waarin men is gehuisvest. Vandaag is dit meer dan 70%. België begeeft zich daarmee tot de koplopers binnen Europa, vergezeld door hoofdzakelijk mediterrane landen. De sterke stijging kwam er mede onder impuls van de verschillende overheden doorheen de jaren, gebruik makend van diverse fiscale stimuli, subsidies en regelgeving. Er zijn verschillende positieve aspecten denkbaar die een stimulering van huiseigenaarschap legitimeren. Echter, vanuit de optiek van dit doctoraal proefschrift, achten we een hoge fractie eigenaars als ongunstig. Het gebrek aan mobiliteit van huiseigenaars heeft namelijk nefaste gevolgen voor de arbeidsmarkt. In deze context verklaarde Andrew Oswald midden jaren '90 de positieve correlatie tussen de graad van huiseigenaarschap en de werkloosheidsgraad. De hoge kosten bij het kopen en verkopen van een woning, leiden tot een lagere geografische mobiliteit van eigenaars tegenover huurders. Wanneer een regio geconfronteerd wordt met een negatieve schok van de vraag naar arbeid, zullen huiseigenaars minder geneigd zijn deze te ontvluchten. De hoge verhuiskosten leiden tot een hoger reserveringsloon en een lagere zoekintensiteit wat betreft jobs buiten de directe omgeving. Op het geaggregeerde niveau gaat volgens Oswald een hogere eigenaarschapsgraad gepaard met een lager effectief arbeidsaanbod, resulterend in hogere lonen en een lagere werkzaamheidsgraad.

Wanneer we de empirische literatuur naderbij bekijken, zien we dat de conclusies grotendeels uiteenvallen volgens het niveau waarop de relatie bestudeerd wordt. Macro studies, die gebruik maken van geaggregeerde data, observeren veelal negatieve effecten van de fractie eigenaars op de arbeidsmarkt. Anderzijds vinden micro studies dat huiseigenaarschap eerder positieve gevolgen heeft voor de individuele arbeidsmarktomstandigheden, of geen significant effect. Vanuit deze ogenschijnlijke tegenstelling, hebben we deze relatie onderzocht voor België vanuit drie verschillende invalshoeken.

Onderzoeksaanpak en resultaten

In de eerste studie van dit proefschrift (Hoofdstuk 2) testen we rechtstreeks de Oswald hypothese, gebruik makend van data op het niveau van de Belgische arrondissementen. We beschikken daarbij over gegevens die zich uitstrekken van 1970 tot 2005. We schatten het effect van het percentage eigenaars op de werkzaamheidsgraad en controleren voor een reeks andere factoren zoals loonkloof, scholing en demografie. We hechten veel aandacht aan enkele methodologische kwesties die in voorgaande onderzoeken vaak onderbelicht bleven. We tonen aan dat de resultaten sterk kunnen afwijken wanneer niet de nodige voorzorgen worden genomen. Onze resultaten bevestigen de Oswald hypothese voor de Belgische arrondissementen. De schattingen tonen aan dat een stijging van het percentage eigenaars met 1 procentpunt, een significante vermindering van de werkzaamheidsgraad met 0,35 procentpunten teweegbrengt. We constateren bovendien dat de omvang van dit effect afhankelijk is van de scholingsgraad binnen het arrondissement. Het Oswald effect is minder sterk als de fractie hooggeschoolden hoger is. Deze bevinding is consistent met de theorie. Hoger geschoolden zullen omwille van de relatief hogere lonen die hen worden aangeboden, minder geremd zijn in hun mobiliteit.

In Hoofdstuk 3 stellen we ons de vraag of aan de onderliggende assumptie van de Oswald hypothese is voldaan. Meer bepaald onderzoeken we of huiseigenaars inderdaad minder makkelijk verhuizen dan huurders. We gebruiken hierbij PSBH data voor de periode 1994-2002 en EU-SILC data voor de recentere periode 2004-2009. We controleren ook voor een hele reeks andere familiale en omgevingsfactoren. In onze analyse worden eigenaars verder onderverdeeld naargelang ze een hypothecaire lening hebben lopen en maken we een onderscheid tussen sociale huurders en private huurders. Onze resultaten suggereren dat eigenaarschap inderdaad een rem is op de verhuismobiliteit. Het negatieve effect is nog sterker in het geval van een hypothecaire lening. Dit is theoretisch te verklaren door de hogere kosten die eigenaars met een lening ervaren bij een verhuis. Deze omvatten bijkomende registratierechten, notaris- en bankkosten. Private huurders hebben de hoogste verhuiskans, gevolgd door sociale huurders. De globale bevindingen bevestigen de onderliggende voorwaarde voor het zich kunnen manifesteren van een Oswald effect.

Hoofdstuk 4 onderzoekt op microniveau de impact van eigenaarschap op de werkloosheidsduur. De EU-SILC data verschaffen informatie over het individueel arbeidsmarktstatuut met een maandelijkse nauwkeurigheid. Voor de periode 2003-2008 beschikken we over een steekproef van 1013 werkloosheidsintervallen. De belangrijkste bijdrage in hoofdstuk 4 is de aanvullende opdeling van huiseigenaars in eigenaars mét en zonder hypotheek. Hoewel dit binnen de empirische literatuur tot heden slechts beperkt aan bod is gekomen, werd deze onderverdeling stevig onderbouwd in recent theoretisch onderzoek. De woonkosten blijken daarbij van primordiaal belang te zijn. Wanneer deze hoog zijn (dus in het geval van hypothecaire afbetalingen), gaat men intensiever op zoek naar werk met een kortere werkloosheidsduur tot gevolg. Eigenaars zonder hypotheek, en dus met lage woonkosten, worden verwacht langer in de werkloosheid te verkeren. Onze resultaten bevestigen deze hypothese en leggen op die manier een rijkere dynamiek bloot dan in de voorgaande papers het geval was. Wanneer geen rekening wordt gehouden met het al dan niet hebben van een hypothecaire lening, is het verschil tussen eigenaars en huurders niet significant. Onze resultaten kunnen mogelijk de tegenstrijdige bevindingen in de voorgaande literatuur verklaren door te wijzen op het verschillend gewicht binnen de steekproef van eigenaars mét en zonder hypotheek.

Besluit

Onze resultaten houden de ogenschijnlijke tegenstelling binnen het empirisch veld grotendeels overeind. Enerzijds concluderen we in Hoofdstuk 2 dat, op basis van geaggregeerde data, huiseigenaarschap heel nefast is voor de werkzaamheidsgraad. Anderzijds blijkt uit Hoofdstuk 4 dat de individuele situatie van huiseigenaars gemiddeld genomen niet verschilt van die van huurders wat de werkloosheidsduur betreft. De resultaten onthullen het belang van woonkosten maar tonen ook aan dat het Oswald effect geen dominante rol speelt op het micro niveau.

Beide conclusies kunnen gerijmd worden door te verwijzen naar de negatieve externe effecten van huiseigenaarschap. Deze effecten zijn niet exclusief van voelbaar door de huiseigenaar zelf maar spelen in op de gehele arbeidsmarkt. Ten eerste zullen eigenaars door de gebrekkige verhuismobiliteit meer geneigd zijn om te pendelen over langere afstanden. De hieruit resulterende verzadiging van het weg- en spoorwegverkeer veroorzaakt extra kosten voor zowel werknemers als bedrijven. Ten tweede leidt een hoge eigenaarschapsgraad tot een uitdunning van de huurmarkt. Het afgenomen aanbod aan huurwoningen remt dan ook de verhuismobiliteit van huurders. Ten derde wordt vaak geargumenteerd dat gebieden met veel eigenaars wantrouwiger staan tegenover lokale bedrijfsinvesteringen uit schrik voor onaangename neveneffecten. Hierdoor zullen bedrijven minder makkelijk de weg vinden naar deze streken wat verder de werkgelegenheid onderuit haalt. Ten slotte volgt uit de theorie dat een verdringingseffect kan optreden. Aangezien huiseigenaars hogere verhuiskosten ervaren, zullen ze meer gemotiveerd zijn om te werken in de eigen streek en zich tevreden stellen met een lager loon. De mogelijkheid bestaat dat hierdoor andere werknemers uit de arbeidsmarkt geprijsd worden. Deze vier tendensen verklaren de mogelijkheid dat het Oswald effect enkel op het macro-economisch niveau waar te nemen is.

In het eerste hoofdstuk van dit doctoraal proefschrift buigen we ons over de mogelijke beleidsimplicaties die naar aanleiding van het geobserveerde Oswald effect kunnen voorgesteld worden. Ten eerste is het wenselijk dat eigenaarschap niet blindweg gepromoot wordt. Een gezonder evenwicht kan bereikt worden indien de overheid ijvert voor *tenure neutrality*. Dit is de toestand waarbij individuen hun beslissing om huiseigenaar of huurder te zijn, enkel laten afhangen van hun persoonlijke voorkeur en niet van overheidsstimuli. Concreet pleiten we voor de afbouw van maatregelen die huiseigenaarschap overmatig promoten, zoals de fiscale aftrek voor hypothecaire woonkredieten. We adviseren om hierbij voorzichtig te werk te gaan om de markt niet overmatig onder druk te zetten en zodoende een te sterke daling van de woningprijzen te vermijden. Anderzijds kan een aanbodbeleid ter bevordering van kwalitatieve huurwoningen de huursector veerkrachtiger maken.

Ten tweede kan het Oswald effect gematigd worden door rechtstreeks in te spelen op de mobiliteit van huiseigenaars. Enerzijds kan in de marge van het actief arbeidsmarktbeleid een maatregel uitgewerkt worden waarbij de overheid een deel van de verhuiskosten op zich neemt indien dit gepaard gaat met tewerkstelling in een andere regio. Anderzijds pleiten we voor een verdere verlaging van de registratierechten en een uitbreiding van het systeem van meeneembaarheid. Beiden zullen de verhuismobiliteit ten goede komen. Bovendien is deze maatregel complementair met het uitdoven van de fiscale aftrek op verschillende vlakken: budgettair, het effect op de woningprijzen, het effect van de relatieve kost van kopen tegenover huren en misschien het allerbelangrijkst… de politieke haalbaarheid. Woonbeleid is een heel gevoelig onderwerp binnen de publieke opinie. De overheveling van de woonbonus naar de regionale overheden in 2014, biedt de kans én de noodzaak om het woonbeleid grondig te hervormen. Het wordt een moeilijke politieke oefening. Laten we alvast hopen dat het nastreven van doelmatig beleid het haalt van de emotionele argumenten. De Belg en zijn baksteen.

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CHAPTER 1

Introduction

Introduction

1. General context and motivation

Homeownership rates have increased strongly in most developed countries since the Second World War. This increase partly resulted from a wide range of policy measures that encouraged the purchase of a house. Many positive externalities of homeownership can be thought of to legitimize these incentives. From a labour market perspective however, the effects of a high fraction of homeowners are less beneficial. The next three chapters of this dissertation analyse the outcomes of housing tenure from this particular perspective. Oswald (1996, 1997) was among the first to elaborately study this relationship. He developed a theory that explains higher unemployment rates in some countries as the result of higher rates of homeownership. A key element in this view is that high costs of buying and selling homes make homeowners less geographically mobile than tenants. When a region is hit by an adverse labour demand shock, it is expected that homeowners are less likely to move. The higher moving cost that they experience, causes higher reservation wages for distant jobs and lowers their search intensity for these jobs. At the aggregate level, a higher number of homeowners implies lower effective labour supply in each region and each labour market segment, with higher wages and lower employment as a result.

Empirical studies reveal a remarkable difference depending on whether they investigate the relationship between homeownership and labour market outcomes at the macro or the micro level. Most macro studies, with some exceptions, support the Oswald hypothesis that higher rates of homeownership in a region or country imply inferior labour market outcomes. On the other hand, most micro studies find that homeowners have better labour market perspectives than tenants, certainly not worse. Given this apparent contradiction, we conduct three studies, analysing the topic from a variety of angles. Chapter 2 of this dissertation analyses the relationship at the aggregate level, more specifically the effect of the share of homeowners on the aggregate employment rate in Belgian districts. In Chapter 3, we focus on the main channel through which the Oswald effect runs. We explain mobility at the individual household level as a function of housing tenure choice amongst a range of control variables. Last, we analyse the effect of homeownership on unemployment duration at the microeconomic level. We test whether homeowners have shorter or longer unemployment spells. Both micro studies also concern Belgium. A review of the three papers is given in the next section, each time discussing the research question, data sources, contributions and conclusions.

2. Research questions, results, and contributions

Chapter 2 is a direct empirical test of the Oswald hypothesis at the macro level. Controlling for a range of other regressors, we measure the effect of the fraction of homeowners on employment. To accomplish this, we use a panel of 42 Belgian districts in the period 1970-2005. The broad time range is unique and enables us to adequately estimate the effects of only slowly changing variables like the share of homeownership, the skill level, and demography. Our results confirm the Oswald hypothesis for Belgium. Estimates show that a 1 percentage point rise in the rate of ownership in a district implies a statistically significant fall in the employment rate by about 0.35 percentage points. As to the determinants of the size of the Oswald effect, we find that it falls in the fraction of high skilled in a district. This result supports the theory described by Dohmen (2005). Because of the better wage perspectives for the high skilled, moving costs will less impede the mobility of this group. We also obtain indicative results that the Oswald effect may be stronger in districts closer to a border, and in districts farther away from major cities and centres of economic activity, but these findings are not statistically significant.

Our main result in favour of Oswald's hypothesis survives various robustness checks. Nonetheless, some of the choices that we make in this paper may require further clarification. First, unlike most of Oswald's analyses, our benchmark model contains the employment rate as dependent variable, instead of the unemployment rate. We prefer to focus on employment for two reasons. First of all, the employment rate has become the main policy objective for labour market performance in Europe since the Lisbon Summit in 2000. In other words, the parameter of interest for policy makers has shifted since the time Oswald launched his hypothesis. Furthermore, the employment rate as performance indicator is much less vulnerable to distortions caused by policy measures (including statistical operations) that serve to mitigate negative labour demand shocks (such as early retirement programs). In this scenario, these shocks are not fully observed by the unemployment rate.

Second, it is plausible that the size of the observed regions has an impact on the estimated effect, yet it is not clear in which direction. The larger the area, the higher the chance that zones with high homeownership rates and zones with low homeownership rates cancel each other out, concealing the effects. On the other hand, when analysing very small cross-sectional units (or in the extreme case individuals, as in the micro studies), one is not able to capture the external effects of homeownership. We have reason to believe that the size of the districts used in our research is adequate. If the external effects would transcend the borders of the districts, we would expect spatial autocorrelation. Tests reveal that this is not the case. Third, because of data constraints, Brussels itself is not included in our panel. Although Brussels is an outlier when it comes to homeownership rate, it is most unlikely that one observation would seriously alter the results. As a last sensitivity analysis, we exclude the districts neighbouring Brussels. This results in an Oswald effect that is slightly larger, which is consistent with the above described interaction of districts close to a major city.

Chapter 3 focusses on residential mobility in Belgium. Using micro data, we estimate the probability of a residential move as a function of housing tenure, area characteristics and household characteristics. The Oswald hypothesis explicitly presumes that homeowners are less mobile than tenants. A longitudinal dataset covering the period 1994-2002 is derived from the Panel Survey for Belgian Households (PSBH). Likewise, we use the European Union Statistics on Income and Living Conditions (EU-SILC) to cover the more recent period 2004-2009. Because the two datasets are not directly comparable, we employ both panels separately. The wide range of socio-economic variables that is provided in the datasets allows us to control for age, family structure, educational level, nationality, income and room stress. The area characteristics are derived from Cambridge Econometrics data and data from the 'FOD Economie', Belgian Federal Government. These imply the provincial unemployment rate, regional dummies and a list of variables capturing housing supply.

We further subdivide tenants as tenants paying market value rent versus 'social' tenants paying a subsidized rent, and homeowners as outright homeowners versus homeowners with a mortgage. Both distinctions are meaningful. Because social tenants have the possibility of losing their privileges when moving, they are expected to be less mobile than private tenants. Furthermore, having or not having a mortgage can influence residential mobility for homeowners in various ways. A mortgage can hamper mobility because of the supplementary transaction costs and the risk for lock-in effects in case of negative equity. On the other hand, Caldera Sánchez and Andrews (2011) argue that the monthly payments of mortgage holders increase the probability of moving, in order to improve their labour market perspectives, and so avoid unemployment. Our results suggest that tenants are the most mobile group, especially those paying market value rent. Homeowners with a mortgage are the least mobile. Although this is in line with most of the preceding studies, earlier results for Belgium revealed no significant difference between outright owners and mortgagees. Methodologically, in this paper we make progress on the existing literature by paying particular attention to (and dealing with) econometric issues such as unobserved heterogeneity and state dependence. However, we also obtain some indications that the strict exogeneity assumption may be violated for some regressors, implying that we cannot exclude the possibility of some bias in our estimated coefficients.

Chapter 4 studies the impact of housing tenure choice on unemployment duration. By using the EU-SILC data for Belgium, we are able to analyse household behaviour in the recent period 2003-2008. The dataset provides detailed information of a person's activity status in each month. We use the spells of unemployment that start after a period of employment (i.e. left-censored spells are withheld). A spell can end with re-employment or with right-censoring. We use a mixed proportional hazard model to estimate the effect of housing tenure on unemployment duration, controlling for a wide range of other variables. We take into account the potential selectivity bias that may arise if a person's unobserved characteristics affect both his unemployment duration and housing tenure. One might falsely interpret the combination of these events as a causal relationship from housing tenure to unemployment. To resolve this issue, we simultaneously estimate a mixed multinomial logit model explaining housing tenure, along the hazard model for unemployment duration. We use instrumental variables (exclusion restrictions) to econometrically identify the housing tenure effect. These are variables that influence housing tenure but do not directly affect unemployment duration. As a first instrument, we adopt the aggregate fraction of homeowners from the study of van Leuvensteijn and Koning

(2004), in our case at the provincial level. Second, we contribute to the existing literature by adding a new instrument, i.e. the relative price of buying the house versus renting in the year of purchase or contract.

Previous research using a similar methodology found evidence for shorter unemployment spells among homeowners than tenants (Munch et al., 2006) or no significant difference between both groups (Battu et al., 2008), and therefore clearly contradicted the Oswald hypothesis. Our main contribution lies in the investigated distinction between housing tenure types, in particular the different types of homeownership. According to the theoretical search model of Rouwendal and Nijkamp (2010), homeowners have shorter unemployment durations when housing expenses are high. These housing expenses are primarily determined by whether the individual has a mortgage or not. Our results show that homeowners with a mortgage have, ceteris paribus, the shortest unemployment spells while outright owners stay unemployed the longest. Tenants take an intermediate position. When we do not make the distinction, no significant difference between owners and tenants is observed, consistent with the results of Battu et al. (2008). Our results demonstrate the relevance of the distinction that we introduce. Two important conclusions emerge from this. First, liquidity constraints and the induced reduction of consumption caused by housing costs, seem to play a more prominent role than mobility constraints. Second, our results suggest that the discrepancy between the findings of the preceding studies, might be the result of a different composition of the group of homeowners in the respective countries that were studied. More specifically, the Danish group of homeowners, studied by Munch et al. (2006) consists of a higher fraction of mortgage holders than the British group of homeowners (Battu et al., 2008) and the homeowners in our sample. The higher weight of this mortgage subgroup may shift the results to better labour market outcomes for homeowners. If data allow, further research is desirable in which liquidity constraints can be taken into account more directly.

3.What it all boils down to: disentangling the apparent contradiction

Our analysis of residential mobility in Chapter 3 proves that homeowners are indeed less residentially mobile than tenants. Apparently they face a higher mobility cost which would make them more vulnerable to unemployment, the so-called Oswald effect. However, Chapter 4 reveals no significant difference in unemployment duration between tenants and the average homeowner. Using a search-theoretic model, Munch et al. (2006) demonstrate the appearance of an alternative link between homeownership and unemployment duration, which may undermine the Oswald effect. According to this model, the effect of a higher reservation wage for distant jobs is partly counterbalanced by the effect of a lower reservation wage for local jobs. It is therefore possible that homeowners have a higher matching probability compared to tenants in the local labour market. Coulson and Fisher (2002) emphasise the importance of social networks in the search for work. Homeowners tend to invest more in their social network which improves their local job opportunities. These arguments provide theoretical grounds for the better or equal labour market performance of homeowners at the micro level. They support the results found in the empirical literature.

The question remains how the at first sight contradictory macro results in Chapter 2 and micro results in Chapter 4 can be reconciled. The answer lays in the external effects of homeownership. We sum up a number of considerations revealing that the negative effects are not necessarily concentrated within the segment of the homeowners. In these cases, being a homeowner does not directly harm the labour market outcomes of the homeowner himself. However, it generates negative effects on the labour market in general. First, as an alternative option to moving, one can commute over a longer distance to ameliorate labour market perspectives. At the individual level, reservation wages increase with commuting distance. Nevertheless, it might still be more favourable than moving if the latter induces high costs. Indeed, as argued in a recent study of Kantor et al. (2012), homeowners accept longer commutes. When the rate of homeownership is high, traffic congestion will increase commuting costs for every individual worker and raise overall production costs for firms. This may further undermine employment. Second, the overall promotion of homeownership might undermine the development of a well-functioning rental market. This might increase moving costs for tenants and hamper the efficiency of the labour market. Third, Blanchflower and Oswald (2013) refer to the possibility of zoning restrictions and NIMBY effects, enforced by the group of homeowners. This might impede business activity and consequently employment. Last, Laamanen (2013) argues that the high search intensity and low reservation wages of homeowners for local jobs, might lead to displacement of other workers in the same region. The net effect for employment at the aggregate level depends on the ratio of the number of displaced workers to the number of homeowners who find new employment. The author provides arguments for the possibility of an increase in the unemployment rate with the fraction of homeownership, in both the short and the long run. These four negative externalities of homeownership explain the possibility that the Oswald effect is observed only at the aggregate level.

4. Policy implications

From the conclusions in the previous sections, we can deduce a number of policy recommendations that can improve labour market outcomes.

- Stop tax deductibility of mortgage payments for new mortgages;
- Decrease transaction costs for buying and selling a house;
- Implement supply-side policies to support the rental market;
- Implement active labour market policies that directly stimulate residential mobility;

Because housing policy in Belgium is a regional matter, it is convenient to concentrate on the Flemish Region throughout this discussion. Let us first target attention at a number of noteworthy characteristics of the Flemish housing policy. An extensive study of the current situation is provided by Heylen and Winters (2012). They list the various policy measures and compare their respective burden on the government's budget. From their data, we can derive that a large extent of the budget is spent on demand-side policies, mainly encouraging homeownership. More specifically, they calculate that in 2012 the total budget to support homeowners was 5.6 times larger than the budget supporting tenants.The main expense is tax deductibility of mortgage payments which accounted for 1400 million in 2012, as opposed to 864 million for all other housing policies together. As of today, this tax deduction is a federal matter but from 2014 onwards it will be the responsibility of the regional level.

Heylen and Winters (2012) also reveal that households with higher incomes receive the highest financial support in case of property acquisition, rendering a so-called Matthew effect. The argument of supporting homeownership as a protection against poverty is therefore not consistent with today's policy. There are two reasons why this inequality emerges. First, it results from the nature of tax deductibility in a progressive tax system. The amount can be deducted from the highest tax bracket, resulting in a higher tax refund for high incomes[1]. Second, the demand-side policies supporting ownership generate a strong incentive to buy a house instead of renting. People who can afford it, will be inclined to become a homeowner regardless of their preferences. This will push up the aggregate homeownership rate. Because the lowest income households benefit more from rent subsidies, this group will be inclined to be a tenant. To sum up, both sides of the income spectrum lack tenure neutrality. Below, we extract a number of suggestions to policy makers, considering the conclusions from this dissertation. Although many other factors might inspire policy makers, our focus remains on the labour market implications of housing tenure choice.

We learned that the perceived Oswald effect in macroeconomic empirical work, is most likely the result of the negative external effects caused by homeowners. In our opinion, there are two major remedies to mitigate the Oswald effect. On the one hand, one can lower the rate of homeowners and on the other hand, one can remove the underlying determinant, the restricted geographical mobility of homeowners.

First, how and to what extent can the rate of homeownership be reduced to a more moderate fraction? For example, policy makers can facilitate supply in qualitative rental housing using subsidies and regulation. However, as long as the incentives to become a homeowner are sustained, the effect might be limited for the simple reason that owners of rental houses may sell them. The rental house may then turn into an owner-occupied house. A more effective path is restoring tenure neutrality. Without doubt, the most conspicuous market distortion is the tax deductibility of mortgage payments. We recommend to eliminate this disproportional stimulus for becoming a homeowner. For two reasons, we advise to retain the benefits granted for existing mortgages, at least to some level or for a certain amount of time. First, the households that bought a house have taken into account the current and future benefits they are entitled to, while making a budget. The unexpected

[1] Since 2012, this is much less the case since a reform was implemented in which the percentage of tax deduction has been fixed at 45%, irrespective of the highest tax bracket.

loss of these benefits might disrupt the household's budget. Second, the elimination of the tax deductibility is likely to have a negative impact on house prices. Because the Belgian housing market (as in many other countries) is characterized by an inelastic supply[2], we can expect that demand-side policies will, to a large extent, be absorbed by fluctuations in the price, especially in the short run. Although this might seem beneficial for future buyers, it is very harmful for current mortgage holders. The negative equity causes so-called lock-in effects. These imply a strong restraint on mobility of this group of homeowners. Also on a larger scale, a strong decline in the average house price can be detrimental for an economy. Therefore, caution is needed. Achieving tenure neutrality is an indispensable objective. Nevertheless, it will only slowly affect the aggregate rate of homeownership.

Second, how can we increase residential mobility? Higher mobility is not only desirable from a labour market perspective but also to achieve a more efficient matching of housing according to household (life cycle related) needs. First, governments can directly encourage mobility by financially compensating the costs that the unemployed experience when moving closer to a new employer. This type of subsidizing can be a complementary tool in the context of activation programs. Within housing policy today, a similar subsidy exists when a household moves to a more adequate residence. It may be useful to implement it for moves to a more adequate labour market as well. Second, a more straightforward policy instrument to stimulate mobility, is directly decreasing the cost of mobility. In 2002, the government of the Flemish region introduced the portability of the transaction taxes. In particular, if an owner-occupier buys a new residence, the transaction taxes paid for the initial residence are deducted from the new transaction taxes. A useful policy measure, because moving costs for the typically immobile homeowners decrease. We recommend to increase the maximum portability and further ease its conditions. Today, the rights expire after two years which is an incentive to remain a homeowner. It is in conflict with tenure neutrality. Additionally, we would recommend reducing overall transfer taxes, which further decreases mobility costs. This will generate the appropriate mobility incentives, as has been empirically proven by van Ommeren and van Leuvensteijn (2005), in the case of the Netherlands.

[2] For estimates of the responsiveness of new housing supply to prices in Belgium and other OECD countries, we refer to Caldera Sánchez and Johansson (2011).

Housing policy attracts a lot of attention from the public opinion. This is not surprising because housing expenses take a large part of the household's budget. Moreover, owning a house is often a very substantial part of the household's total wealth. Changes in housing policy are therefore a very sensitive subject matter for the vast majority. From the four policy recommendations, two are likely to have a large impact on household's behaviour: eliminating tax deductibility of mortgage payments and reducing transaction costs. We believe that these two measures are complementary for four reasons. First, only lowering transaction costs would further encourage homeownership. This effect will be outweighed by the dissuasive effect of the ceased tax deductibility. Second, the expected drop in house prices caused by the latter, will be counterbalanced to some extent by the first. As described above, a very strong correction in house prices is harmful for mortgage holders. Furthermore, it can impair the economic system. Third, both policy measures are compatible for the government's budget. As shown above, the tax deduction is a heavy burden on the budget. Ending it will generate room to cut transaction taxes and imply other measures improving the quality and sustainability of the housing stock. Last, the momentum is just right. As the tax deductibility for mortgage payments becomes a regional matter, it offers the Flemish Government the opportunity to rethink its housing policy. Choices have to be made because budget constraints force the policy makers to do so. Ceasing the tax deductibility will require political decisiveness. Cutting transaction costs may support its political feasibility.

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CHAPTER 2

Houses and/or jobs: Ownership and the labour market in Belgian districts

Houses and/or jobs: Ownership and the labour market in Belgian districts

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Abstract

A.J. Oswald argues that high rates of homeownership may imply inferior labour market outcomes. Using a panel of 42 Belgian districts since the 1970s and accounting for other key determinants of employment, this paper confirms the Oswald hypothesis. A 1 percentage point rise in the rate of ownership in a district implies a statistically significant fall in the employment rate by about 0.35 percentage points. This negative effect declines in the fraction of high-skilled in a district. Our results underscore the importance of controlling for unobserved district-specific fixed effects and common time effects, and of appropriately dealing with endogeneity.

JEL classification: E24, R23
Keywords: employment, homeownership, Oswald hypothesis, Belgian districts, panel data

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1. Introduction

Throughout recent history, governments in many countries have encouraged homeownership. Ownership is seen as a secure way for the population to accumulate assets. Moreover, ownership generates significant social benefits. Owners are more likely to have long residence spells, which contributes to local neighbourhood stability and to the accumulation of social capital (DiPasquale and Glaeser, 1999; Rohe et al., 2002; Dietz and Haurin, 2003; Engelhardt et al., 2010). Renters do not bring about the same returns due to their higher degree of geographical mobility. From a labour market perspective, however, rising degrees of homeownership are much more controversial. Homeownership may restrict geographical mobility, and imply inferior labour market outcomes, both for the individual and in the aggregate. Oswald (1996, 1997a,b,c) was among the first to advance this argument. If demand for labour falls in a region, homeowners will be less inclined to move to more prosperous regions mainly due to high costs of selling and buying homes. Renters by contrast can move at much lower cost. In equilibrium, higher degrees of homeownership imply higher unemployment. Empirically, Oswald's evidence in favour of his hypothesis relies mainly on cross country macroeconomic data, and on aggregate data for regions within individual countries. Oswald (1996) observed higher unemployment rates in OECD countries with a higher fraction of owners (versus renters). Also, he found that since the 1970s the unemployment rate increased most in those countries with the strongest growth in the rate of ownership. According to his results, an increase of the rate of ownership with 10 percentage points causes an increase of the unemployment rate with 2 percentage points.

The Oswald hypothesis has provoked a large body of theoretical and empirical work. Coulson and Fisher (2009) show in a survey that a change of theoretical assumptions may generate results that differ from Oswald's, both for individual homeowners and for the aggregate labour market. Empirically, a wave of studies has not settled the issue, although it may be possible to observe some structure in the results. Studies using microeconomic data very often challenge the Oswald hypothesis, and find homeowners to have better employment positions (e.g. Coulson and Fisher, 2002, 2009; Robson, 2003; van Leuvensteijn and Koning, 2004; Munch et al., 2006, 2008). Studies using macroeconomic data are more often in line with Oswald (e.g. Partridge and Rickman, 1997; Pehkonen, 1997; Nickell, 1998; Nickell et al. 2005; Cochrane and Poot, 2007), although various researchers obtain dissident or insignificant results (Flatau et al., 2002; Barrios García and Rodríguez Hernández, 2004, Coulson and Fisher, 2009). Overall, it is difficult however to draw convincing conclusions from these macro studies due to their imperfect or limited econometric setup.

This paper tests the Oswald hypothesis in a panel of 42 Belgian districts ('arrondissements') since the 1970s[1]. Along the time dimension we have data for six years between 1970 and 2005. Our dependent variable is the employment rate, the fraction of working age population with a job. Our approach and data availability are such that this may be the first paper to avoid three limitations in existing empirical macro studies. *First*, many macro studies lack data along the time dimension, which makes it impossible to control for unobserved fixed effects, and which may lead to seriously biased estimates. The availability of data since the 1970s enables us to control for fixed effects. Moreover, it allows us to include in this study the periods with most labour market turbulence since the Second World War, and to embed the Oswald hypothesis in a broader model including various other determinants of employment like labour costs and productivity, skill level of the population and demographic variables. If the time dimension is short, and data availability limited, it clearly becomes difficult to estimate the effects of only slowly changing variables like the rate of ownership, the skill level, and demography. Only Oswald (1996, 1997a, 1997b), Partridge and Rickman (1997), Green and Hendershott (2001) and Nickell et al. (2005) exploit data for the 1970s and the 1980s. *Second,* most existing macro studies neglect the possibility of reverse causality. Yet, due to the potential influence of employment in a region on permanent income and tenure choice of households in that region, ownership may be endogenous to changes (shocks) in employment. If not dealt with, positive correlation between shocks to employment and the rate of ownership may bias the estimated Oswald effect upwards. Empirically, this would impose the use of IV techniques. We employ these in this paper. Barrios García and Rodríguez Hernández (2004), Cochrane and Poot (2007) and Coulson and Fisher (2009) are the only studies we know to have used IV-methods before. *Third*, observing the Oswald effect is (only) one thing. Another may be to understand what determines its size, and economic significance. Our relatively large panel along both the

[1] Appendix A contains a map and some more information on these districts.

cross-sectional and time dimension allows us to test various interaction effects which may shed light on this. We test the role of structural geographic and schooling related variables. Among these variables are the proximity of a border (country or language border), population density, the skill level of the population, etc.

Our main findings are as follows. We find evidence confirming Oswald's hypothesis for Belgium. We observe that a 1 percentage point rise in the rate of homeownership in a district implies a statistically (and economically) significant fall in the employment rate by about 0.35 percentage points. Our results show the importance of controlling for both cross-sectional fixed effects and common time effects. If we do not do this, the estimated Oswald effect can be totally different, highly insignificant, close to zero and sometimes even positive. Our results also demonstrate that the estimated Oswald effect may be biased when endogeneity of homeownership is disregarded. Not using IV techniques, we find a much smaller (less negative) Oswald effect. As to the determinants of the size of the Oswald effect, we find that it falls in the fraction of high skilled in a district. We also obtain indicative results that the Oswald effect may be stronger in districts closer to borders, and in districts farther away from major cities and centres of economic activity, but these findings are not statistically significant.

Our main result in favour of Oswald's hypothesis survives various robustness checks. These concern changes in the imposed functional form of the relationship between ownership and employment, changes in our panel along the time dimension, and changes in the dependent variable. Changing our focus to unemployment rather than employment, does not affect our main conclusion.

Among our other results, we observe negative effects on the employment rate in a district of the ratio of wage costs to productivity, and (insignificant) positive effects of the fraction of high skilled. We also see a (time-varying) influence of some demographic variables, like the age structure of the population. Often, however, this influence is not statistically significant either.

In the following section of the paper we briefly review the existing theoretical and empirical literature on the relationship between housing and jobs. The third section describes our econometric model. Our analysis of equilibrium employment is situated within the New

Keynesian competing claims approach developed mainly by Layard et al. (1991). Here we also take into account specific characteristics of wage setting in Belgium. To define instruments for the rate of homeownership in the employment regression we build on the literature on tenure choice initiated by Rosen (1979) and Rosen and Rosen (1980). The fourth section describes our dataset. The fifth section presents the results of our econometric analysis. We summarize our main findings in the final section.

2. Homeownership and employment: a brief review of the literature

(Un)employment rates differ widely across regions in most countries, including Belgium. Geographical mobility can be a vigorous instrument to eliminate these differences by shifting labour supply from high to low unemployment regions. Theoretically, higher wages or a higher probability to find a (suitable) job in prosperous areas could bring about this shift. Empirical evidence shows that the latter is the most important motivation for workers to be mobile (Blanchard and Katz, 1992; Böheim and Taylor, 2002). However, whether an economic agent decides to work in another region depends not only on expected benefits. Moving also generates costs: search and transaction costs when selling and buying a house, commuting costs, costs to overcome cultural or language barriers, personal costs when leaving familiar surroundings, etc.

Oswald (1996, 1997a,b,c) emphasizes the negative effects of homeownership on geographical mobility and labour market performance. Oswald (1997c) describes a perfectly competitive economy with two separate locations that are joined by a road. People have to live in one of them, either as owner or as renter. Each location experiences real shocks to labour demand. Tenure choice is made before these shocks are revealed. When their region is hit by a bad shock, renters can move to the other region at no cost. Owners in the bad region either remain unemployed and accept unemployment benefits, commute to the better region at a commuting cost, or pay a fixed (high) transaction cost and move. The commuting cost rises in the number of commuters. At some number of commuters this cost becomes equal to the transaction cost of moving. Due to commuting or moving costs, owners will have a higher reservation wage for jobs in the other location. As a result, labour supply to each location is horizontal at a low level of wages up to the number of owners in that location and the total number of renters in the economy[2]. It then becomes upward sloping as higher wages will be necessary to induce (rising numbers of) owners from the other location to commute. Labour supply becomes horizontal again when commuting costs have risen to the level of the transaction cost of moving. Everyone is willing to work in a good region at a wage that covers both the unemployment benefit and moving costs. In the end the position of the labour demand curve determines equilibrium quantities and wages. Given the competitive nature of the labour market, owners and renters receive the same wage offers. Due to their higher reservation wage at distant jobs, however, owners are more likely to be unemployed. Renters are fully employed. Furthermore, at the aggregate level, higher degrees of homeownership imply a leftward shift of the upward sloping part of the labour supply curve. Lower equilibrium employment, higher unemployment and higher wages are the result.

Oswald's arguments may be strengthened by a number of complementary considerations. First, if long distance commuting contributes to traffic congestion, overall production costs may rise, which further undermines employment. Hymel (2009) provides empirical proof of congestion damping employment growth in U.S. metropolitan areas. Furthermore, if the overall promotion of homeownership undermines the development of a well-functioning rental market, it will also be more difficult for unemployed renters to move to other regions (Oswald, 1999). Traffic congestion and a tight rental market imply that the disadvantages of homeownership are not necessarily concentrated in the segment of owners.

Nickell (1998) and Nickell and Layard (1999) embed Oswald's argument in an imperfectly competitive macro model of the labour market. In this model equilibrium (un)employment reconciles competing claims of wage and price setters. Any factor which raises targeted price or wage mark-ups will imply higher equilibrium unemployment. An important determinant of the price mark-up is the degree of product market competition. Wage mark-ups depend on the unemployment benefit system, union power and the characteristics of wage bargaining, labour taxes, etc. Ownership is important in this setup as a determinant of wage pressure. Following Oswald, rising rates of ownership imply reduced

[2] This low level equals the level of the unemployment benefit (or the value of leisure).

mobility and search effectiveness among the unemployed. The employed can then claim a higher wage mark-up. Ownership may also raise the mark-up of prices on wages because non-wage costs may rise: hiring costs (if it becomes more difficult to fill in vacancies), congestion costs, etc. Overall, equilibrium employment will fall.

More recent theoretical work has reconsidered and/or extended Oswald's assumptions and conclusions. Dohmen (2005) basically confirms Oswald's results but emphasizes the role of education and skills. Workers only move to another region in Dohmen's model when the wage in that region exceeds the unemployment benefit and the cost of changing location. Since the latter cost is higher for homeowners, owners will be less mobile and face higher unemployment, as in Oswald. Rising ownership rates then go along with inferior labour market performance. Skill differences may however disturb this simple pattern. High skilled workers earn high wages, which exceed the unemployment benefit plus relocation cost. As a consequence, the skilled may both be owner and mobile. Their mobility raises their chances to find a job. The low skilled, however, earn wages below the sum of the unemployment benefit and the cost of changing location. As a consequence, when a low skilled owner loses his job, he will not move, and remain unemployed. Low skilled renters by contrast remain mobile. The implication of Dohmen's model for empirical work is important. When testing the relationship between ownership and labour market outcomes, it is crucial to control for skill levels, i.e. to keep skills constant in the regression. Furthermore, above a certain skill level, there need not be any relationship between ownership and employment.

Munch et al. (2006) raise another argument which may undermine the Oswald hypothesis. Due to high costs of moving, owners will not only have a higher reservation wage for distant jobs, they will also have a lower reservation wage for local jobs. It is therefore possible that rising ownership goes along with higher employment, but then this should be at lower wages. Brunet and Havet (2009) confirm this idea for French workers. Homeowners in their study are more wage downgraded (and feel more overeducated) than renters. In line with this, Rouwendal and Nijkamp (2010) find empirical prove for lower geographical mobility of homeowners but also for higher exit rates from unemployment. The latter is due to more intensive search activity and faster acceptance of jobs on the local labour market, especially by highly leveraged owners. Munch et al. (2008) add that increased willingness to accept local jobs need not imply lower actual wages. Immobility may cause owners to invest more in their local jobs, increasing firm-specific productivity. The establishment of a long-term employment relationship may also raise the incentive for firms to train their workers-owners.

Coulson and Fisher (2009) discuss the Oswald hypothesis within a model of search and bargaining in the style of Pissarides (1990). Owners face higher unemployment than renters in this model because they search on a smaller scale. Because their search is narrower, owners have less bargaining power, which implies that firms can make them work at lower wages. The latter effect is important because it implies that an aggregate rise in ownership reduces expected wages and raises expected profits for firms. Higher expected profits may cause new firms to enter. Under certain assumptions this favourable entry effect may dominate the unfavourable (standard) composition effect according to which an increase in the number of (immobile) owners undermines overall labour market performance.

Theoretical ambiguity underscores the relevance of empirical work on the Oswald hypothesis. Empirical studies do not settle the issue, however, certainly not when it comes to aggregate effects. Among studies that make use of micro data one can observe some degree of consensus. Most of these studies find homeowners to have a better employment status than renters (see e.g. Coulson and Fisher, 2002, 2009, for the US; Robson, 2003, for the UK; van Leuvensteijn and Koning, 2004, for the Netherlands; Munch et al., 2006, 2008, for Denmark). Owners are in general less mobile than private renters (Caldera Sánchez and Andrews, 2011). However, when they lose their job, this does not necessarily imply longer unemployment spells. Battu et al. (2008) observe similar unemployment durations for homeowners and private renters in the UK. Munch et al. (2006) find homeowners in Denmark to have even shorter unemployment spells due to a lower reservation wage to local jobs.

Empirical studies using macro data cannot confirm the message emanating from the (more or less) micro consensus. Many macro studies confirm the Oswald hypothesis that a rise in the rate of homeownership goes along with inferior labour market results (see Table 1 for an overview). The question is how strong and robust this finding is. On the one hand, a contradiction between micro and macro findings is perfectly possible. As we have mentioned before, rising degrees of homeownership may cause negative effects (congestion, tightening

Table 1. Empirical studies of the Oswald hypothesis using macro data.

Study	Regions or countries (# cross-sections)	Time dimension	Methodology	Oswald?
Oswald (1996)	US states (51) UK regions (13)	1986-95, annual 1973-94, annual	Panel data. Fixed Effects OLS with time dummies. Bivariate (+ lags), levels	yes
	regions within France (22), Italy (20) and Sweden (8)	1990s (1 observation)	Correlation between levels in unemployment and ownership, bivariate	yes
	cross-section of countries (11, 18)	1960s (1 observation), 1990s (1 observation)	Correlation between levels in unemployment and ownership, bivariate	yes
Oswald (1997a)	US states (51)	change between 1970s and 1990s (1 observation)	Correlation between changes in unemployment and ownership, bivariate	yes
Oswald (1997b)	regions within OECD countries	1990s (1 observation)	Correlation between levels in unemployment and ownership, bivariate	yes (a)
Partridge and Rickman (1997)	US states (48)	1972-1991, annual	Panel data. Pooled OLS / Fixed Effects OLS with year dummies, multivariate	yes
Pehkonen (1997)	regions within Finland (13)	1991 (1 observation)	OLS, bivariate, multivariate	yes (b)
Nickell (1998)	OECD countries (20)	Mid 1980s and early 1990s (1 observation per period)	Panel data. Random Effects GLS, multivariate	yes
Hassink en Kurvers (2000)	regions within the Netherlands (COROP) (40)	1990-1998, annual	Panel data. Pooled OLS / Fixed Effects OLS, deterministic time trend. Bivariate (+ lags), levels	no
Green and Herdershott (2001)	US states (51)	change between 1970 and 1990 (1 observation)	Bivariate regression, WLS, different unemployment rates (age groups)	yes/no (c)
Flatau et al. (2002)	Regions (LGA) within Australia (590)	1986, 1991, 1996, 2001 (4 observations)	Multivariate regression, WLS, separate regressions per year	no
Glaeser and Shapiro (2003)	US Metropolitan Statistical Areas	1998 (1 observation)	Correlation between levels in unemployment and ownership, bivariate	no
Barrios García and Rodríguez Hernández (2004)	regions within Spain (46)	1991 (1 observation)	Cross section. Multivariate, Simultaneous equation system explaining unemployment and home ownership, 3SLS	no
Nickell et al. (2005)	OECD countries (19)	1961-1995, annual	Panel data. Fixed Effects GLS with year dummies, multivariate	yes
Bassanini and Duval (2006)	OECD countries (21)	1982-2003 (1 observation)	Bivariate regression on the fixed country effect in a panel study of unemployment on home ownership	yes
Cochrane and Poot (2007)	regions within New Zealand (58)	1986, 1991, 1996, 2001 (4 observations)	Panel data. Pooled OLS / Fixed Effects OLS / Hausman Taylor estimator. Multivariate	yes
Coulson and Fisher (2009)	US Metropolitan Statistical Areas	1990 (1 observation)	OLS and 2SLS, multivariate	no
Lerbs (2011)	regions within Germany (87)	1998, 2002 and 2006 (3 observations)	Separate regressions per year (OLS), Panel data: pooled OLS, Fixed Effects OLS	yes/no (d)

Notes:

(a) except for Belgium, the Netherlands and West Germany

(b) significant only in the bivariate case

(c) no for young and older households, yes for middle aged

(d) no for OLS and pooled OLS, yes for Fixed Effects of rental markets, bargained wage pressure,…) beyond the owners themselves. Even if they are not worse off, aggregate labour market performance may be weaker. Clearly, the aggregate story is important for policy makers. On the other hand, many macro studies may be challenged on methodological grounds. Our summary in Table 1 reveals that many studies have only one observation along the time dimension which makes it impossible to control for fixed regional/country effects. This also makes it more difficult to embed the Oswald hypothesis in a broader model explaining (un)employment, where also differences in wages and productivity, skills, demography, etc. have their role. Furthermore, only Barrios García and Rodríguez Hernández (2004), Cochrane and Poot (2007) and Coulson and Fisher (2009) control for endogeneity of homeownership by means of IV methods. Yet, both theoretically and empirically, housing tenure choices have been found to be determined also by one's employment prospects and permanent income (e.g. Rosen and Rosen, 1980; Henley, 1998; van Leuvensteijn and Koning, 2004). Neglecting the possibility of reverse causality could bias the estimates. In the next sections we try to overcome these limitations in an empirical macro study for Belgium.

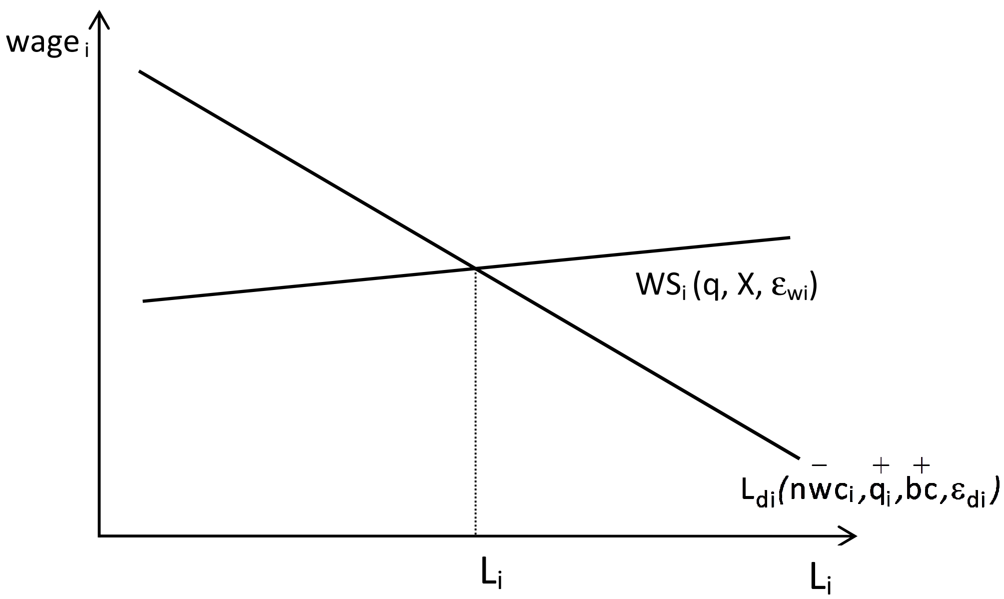
3. Econometric model and methodology

We now discuss our empirical specification for the employment rate and some methodological considerations, which guide our analysis in the next sections. We define the employment rate as the fraction of all people at working age living in a district who have a job. Our setup is mainly inspired by Layard et al. (1991) and Nickell and Layard (1999). Their approach to model the determination of wages and employment corresponds most closely to the Belgian situation. We rely on Oswald (1997c) and some of the literature that we summarized in the previous section when it comes to the effects of changes in ownership on employment.

3.1 Empirical specification

Starting point of our discussion is Figure 1, describing the labour market and the determination of the equilibrium number of jobs in district i. The latter is obviously a key determinant of the employment rate among the people at working age living there. It need not be the same, however, since people may commute to work in a different district. The equilibrium number of jobs in the district (L_i) is determined at the intersection of the labour demand curve (L_{di}) and the wage setting curve (WS_i). Labour demand falls in the real wage per worker ($wage_i$), including taxes on labour. For a given real wage, labour demand is negatively affected by real non-wage production costs (nwc_i) and positively by labour productivity (q_i). Business cycle and other aggregate labour demand shocks are captured by bc, district-specific demand shocks by ε_{di}. The wage setting curve (WS_i) indicates bargained real wages. It is flat since wages in a district are only very weakly affected by local employment conditions. Wages in Belgium are mainly bargained at the sectoral level, often within a nationally imposed range. The coverage rate of collective bargaining exceeds 90% (OECD, 2004). Wages will therefore mainly reflect sectoral and national variables, like sectoral or aggregate labour productivity (q) and overall wage push variables (X). The latter include union power, unemployment benefits, the tax wedge, etc. As mentioned before, Nickell (1998), Nickell and Layard (1999) and Nickell et al. (2005) also see a role for aggregate ownership here. The role for local factors shifting the WS-curve (ε_{wi}) will be very small.

<u>Figure 1</u>. Employment (number of jobs) and wages at the district level



Equation (1) puts these theoretical considerations into a workable econometric specification for the employment rate in district i and year t. As we indicated below the equation, our dataset contains observations for 42 districts over 6 years between 1970 and 2005 (see next section).

$$\text{Empl}_{it} = \delta_i + \gamma_1\,\text{OWN}_{it} + \gamma_2\,\text{Schooling}_{it} + \gamma_3\,\log(\text{wage}_{it}) + \gamma_4\,\log(q_{it}) + \gamma_5\,\text{Age1524}_{it}$$

$$+ \gamma_6\,\text{Age5564}_{it} + \gamma_7\,\text{DFlanders90}_{it} + \lambda_t + \upsilon_{it} \qquad (1)$$

with : $i = 1,\ldots,42;\quad t = 1970,\ 1977,\ 1981,\ 1991,\ 2001,\ 2005.$

The parameters γ_3 and γ_4 measure the effects on the employment rate in percentage points of a 1 percent increase in the real wage (wage_{it}) and labour productivity (q_{it}) respectively. We expect γ_3 to be negative and γ_4 to be positive. The main reasons to have the rate of ownership (OWN_{it}) in Equation (1) – for given wages and productivity – follow from our discussion in the second section. They are as follows. First, ownership may affect non-wage labour costs for firms in a district (nwc_{it}) due to an increase in traffic and congestion costs. Labour demand may shift to the left. Second, the rate of ownership may affect the reservation wage, search intensity and overall mobility of inhabitants in the district. Owners may have a lower reservation wage, and search more intensively, for local jobs. Given the nature of wage bargaining in Belgium, the influence on wages is likely to be very small. The probability for firms to fill vacancies, however, may rise, which brings down non-wage labour costs (hiring costs), and promotes employment. The third effect of ownership concerns the employment rate (Empl_{it}) for a given number of jobs in the district (L_{it}). As argued by Oswald (1997c), owners also have a higher reservation wage for distant jobs. For a given level of wages in other districts, they may therefore have a higher probability than renters to be unemployed. The aggregate employment rate in their own district will then fall, as a smaller fraction of the population will have a job. Which of all these effects from ownership is dominant, and therefore the sign of γ_1, remains an empirical issue.

Other determinants of the employment rate in Equation (1) are the skill level of the population (Schooling_{it}), two demographic variables (Age1524_{it}, Age5564_{it}) and a separate dummy for all districts in one region (Flanders) since 1990 (DFlanders90_{it}). As we explain below, we measure the skill level by the fraction of people with a tertiary degree. Productivity already being controlled for by including q_{it}, 'Schooling' mainly captures the idea raised by Dohmen (2005). For a given share of owners in a district, overall mobility of the population and expected employment rates will rise when skill levels are higher. High skilled workers are better able to bear commuting costs, for example. As demographic variables we consider the share of two specific age groups among the population. Our selection of groups reflects well-known differences (in all OECD countries) in labour market participation and unemployment rates among young, prime-age and older workers. We expect the employment rate in a district to be lower when the fractions of the youngest people (age 15-24) and the older people at working age (age 55-64) rise, implying negative signs for γ_5 and γ_6[3].

The introduction of a separate Flemish regional dummy since 1990 (DFlanders90) captures the possible effects from constitutional reform in Belgium. Since the end of the 1980s the Flemish and Walloon regions have gained much more autonomy in the area of economic policy, including important aspects of labour market policy (e.g. public employment services and training of the unemployed). The parameter γ_7 measures differential effects for all districts in the Flemish region. Finally, we control for district-specific fixed effects (δ_i) and common time effects (λ_t). The latter capture the effects of common labour demand shocks (e.g. aggregate business cycle effects, oil shocks). Idiosyncratic shocks will show up in υ_{it}.

Next to its basic specification, we estimate in the fifth section two extended versions of Equation (1). In a first extension, we allow for time variation in the effects of the demographic age groups. Extension of compulsory education from the age of 14 to 18 in Belgium since 1983 for example may induce lower employment for a given demographic structure. Employment rates may also be affected when preference for leisure or non-employment benefit regimes evolve differently across age groups. In this respect, the increased possibility to retire early since the end of the 1970s may explain lower employment rates among older workers in the second half of our period of study. Our second extension aims to shed more light on the determinants of the size of the Oswald

[3] Note though that these expectations are unconditional. Controlling for (tertiary) schooling, and wages and productivity, expected signs may be less straightforward.

effect. To that aim we introduce in Equation (1) a number of interaction terms γ_{11} VAR*OWN_{it}, where VAR is a variable that may affect the size of the Oswald effect. This variable may vary along the time dimension or the cross-sectional dimension. Variables that we have in mind are the skill level of the population, population density, the proximity of a country or language border, and the proximity of a major centre of economic activity.

3.2 Methodological considerations

Methodologically, it is obvious from Figure 1 and from the literature on the determinants of ownership that instrumental variables techniques will be necessary to estimate Equation (1). Figure 1 reveals the endogeneity of real wages in a district to district-specific shocks in labour demand. Positive shocks will push up wages, and induce correlation between υ_{it} and $wage_{it}$. Given the above mentioned characteristics of wage formation in Belgium, reflected in the flat slope of the WS-curve, this kind of endogeneity is most likely very small, but it will not be zero. Furthermore, any labour demand shock affecting employment and the error term in Equation (1) may also feed through in district-specific productivity q_{it}. As to ownership, its endogeneity is clear from work on tenure choice by e.g. Rosen (1979) and Rosen and Rosen (1980). Micro tenure choice is commonly modelled as a function of the relative cost of living as an owner versus living as a renter, household permanent income, and a number of social and demographic characteristics of the household. The employment situation being a key determinant of permanent income, the proportion of homeowners in the population is logically affected by the (un)employment rate (see also Di Salvo and Ermisch, 1997; Barrios García and Rodríguez Hernández, 2004). Finally, also 'Schooling' may be endogenous to shocks in employment. The literature for example provides ample empirical evidence that schooling is counter-cyclical (e.g. DeJong and Ingram, 2001; Heylen and Pozzi, 2007). Positive shocks to employment may pull young people out of education and into work, and vice versa. We discuss our choice of instruments in the fifth section.

Another methodological issue follows from the spatial dimension of our dataset and the possibility of spatial autocorrelation. If significant, we would need to take this into account in our estimation. To test for spatial effects, we computed Geary's C statistic on the dependent variable as well as on the residuals for each single year *t*. We could not reject the null hypothesis of no spatial autocorrelation[4].

4. Data

We use macro data at the level of Belgian districts. Because of some difficulties in data consistency, and because of its different nature, we have omitted the Brussels district. This leaves us with 42 cross-sections, 22 in Flanders and 20 in the Walloon Region (see Appendix A). As to the time dimension, we are limited to the years in which a census or a large-scale survey has taken place. The years in our database are 1970, 1977, 1981, 1991, 2001 and 2005. Since in 2005 the survey only took place in Flanders, we are left with a panel of 232 observations. In this section we describe our data. We summarize the main descriptive statistics of all variables in Table 2.

Figures 2 to 5 show the evolution of important variables graphically. To bring some structure - it is not practical to show data for all 42 districts - we select those Flemish and Walloon districts that are at the 20th, the 50th and the 80th percentile when ranked from low to high according to the change in the employment rate since 1970. So, these are relatively weak, median and relatively strong performers when it comes to change in the employment rate. In Flanders these districts are respectively Gent, Turnhout and Brugge, in Wallonia Tournai, Nivelles and Waremme. For a detailed description of the construction of our data and their sources, we refer to Appendix B.

Figure 2 shows the evolution of the employment rate. We observe a fall in about all districts during the 1970s. In Wallonia employment continues to decline on average during the 1980s, whereas in Flanders it then recovers. During the 1990s and 2000s most Belgian districts show rising employment rates. Figure 3 depicts the evolution of the rate of homeownership. This rate represents the fraction of houses that are occupied by their owner. The remaining fraction is occupied by renters. We observe a gradual increase in

[4] None of the values for Geary's C that we obtain, differ significantly from 1 (p-values are always above 0.25). In the same spirit, we also tested for temporal autocorrelation in the residuals. Since our panel data are unequally spaced along the time dimension, we relied on the non-parametric Runs test. Here also, test results could never reject the null hypothesis of no autocorrelation. Details on all these tests are available upon request.

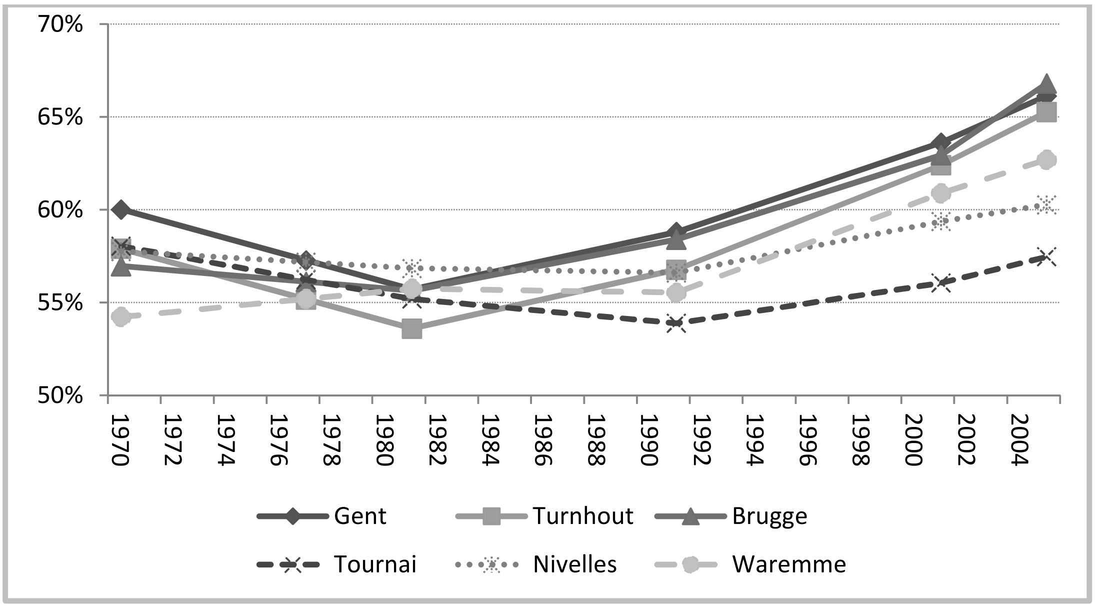
ownership in about all districts, although the size of this increase clearly differs across districts. As to skill levels (Schooling) we were able to detract from the censuses the population of age 14 and older that has terminated school, sorted by their highest diploma. For our regressions we use the number of highly skilled people, i.e. people with tertiary education, in percent of the population of 14 and older. Figure 4 shows the data for the six districts that we focus on. We observe a rise in each of them. Compared to other variables, differences across districts are quite small for this variable. The data that we report in Table 2 show that, relative to the within standard deviation, the between standard deviation is the smallest for Schooling.

Table 2. Main descriptive statistics of the variables

	EMPL	OWN	SCHOOLING	WAGE	PRODUCTIVITY (q)	WAGE GAP	AGE1524	AGE2554	AGE5564
Overall Mean	57.2%	69.7%	14.5%	1.505	1.286	1.198	14.3%	39.5%	10.8%
Minimum	44.0%	44.3%	5.3%	0.698	0.601	0.891	10.7%	32.7%	7.1%
First quartile	54.0%	64.7%	9.0%	1.279	0.963	1.100	12.6%	37.2%	10.3%
Median	56.8%	70.8%	12.5%	1.582	1.175	1.191	14.5%	39.5%	11.0%
Third quartile	60.0%	75.5%	20.0%	1.766	1.528	1.346	15.7%	41.7%	11.4%
Maximum	70.4%	83.3%	38.7%	2.298	2.424	1.518	20.6%	45.0%	14.9%
Std. Dev.	4.8%	7.8%	6.7%	0.381	0.413	0.160	1.9%	2.9%	1.0%
Between Std. Dev.	3.6%	6.7%	2.5%	0.156	0.214	0.121	1.8%	2.7%	0.7%
Within Std. Dev.	3.2%	4.1%	6.2%	0.348	0.354	0.106	0.7%	1.1%	0.7%
1970 mean	56.6%	63.8%	8.3%	0.882	0.803	1.108	14.0%	39.3%	11.0%
1977 mean	55.2%	68.8%	9.2%	1.320	1.012	1.319	14.5%	39.2%	10.9%
1981 mean	54.4%	68.4%	9.7%	1.433	1.095	1.320	14.7%	39.5%	10.5%
1991 mean	55.7%	71.2%	14.9%	1.663	1.405	1.198	14.4%	39.9%	10.8%
2001 mean	59.4%	72.9%	21.2%	1.857	1.654	1.146	14.0%	40.0%	11.0%
2005 mean	61.9%	75.7%	23.7%	1.876	1.746	1.099	14.4%	39.2%	10.7%
Observations	252	232	252	252	252	252	252	252	252

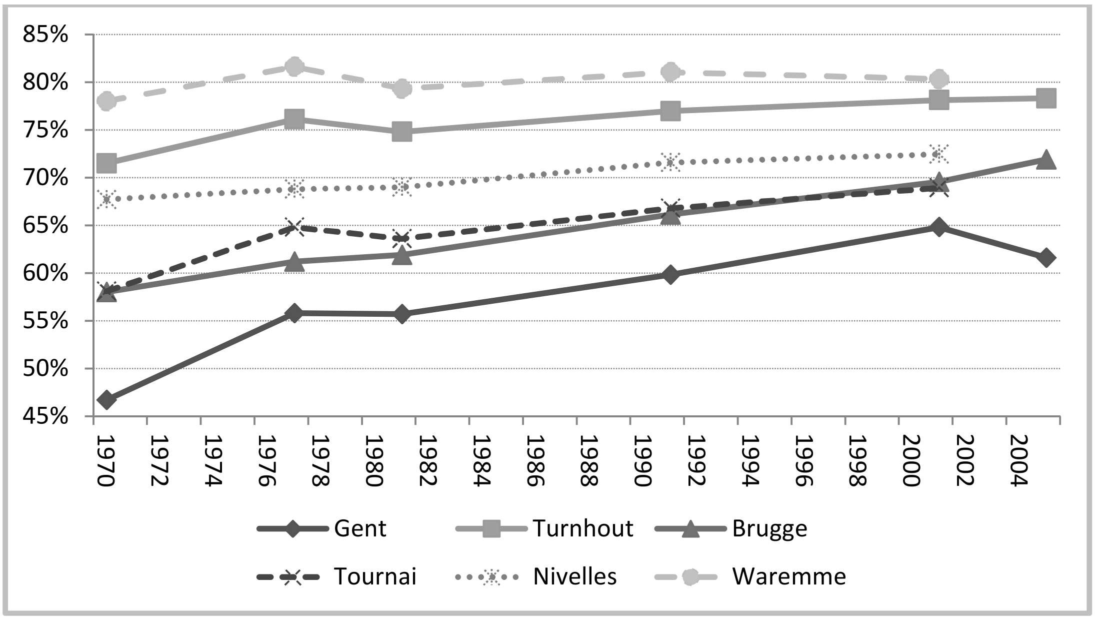
Source: see Appendix B. Note that the data for OWN in 2005 only include Flemish districts.

Figure2. Employment rate among 15-64 year olds living in the district (Empl)



Source: Appendix B.

Figure 3. Homeownership rates (OWN)



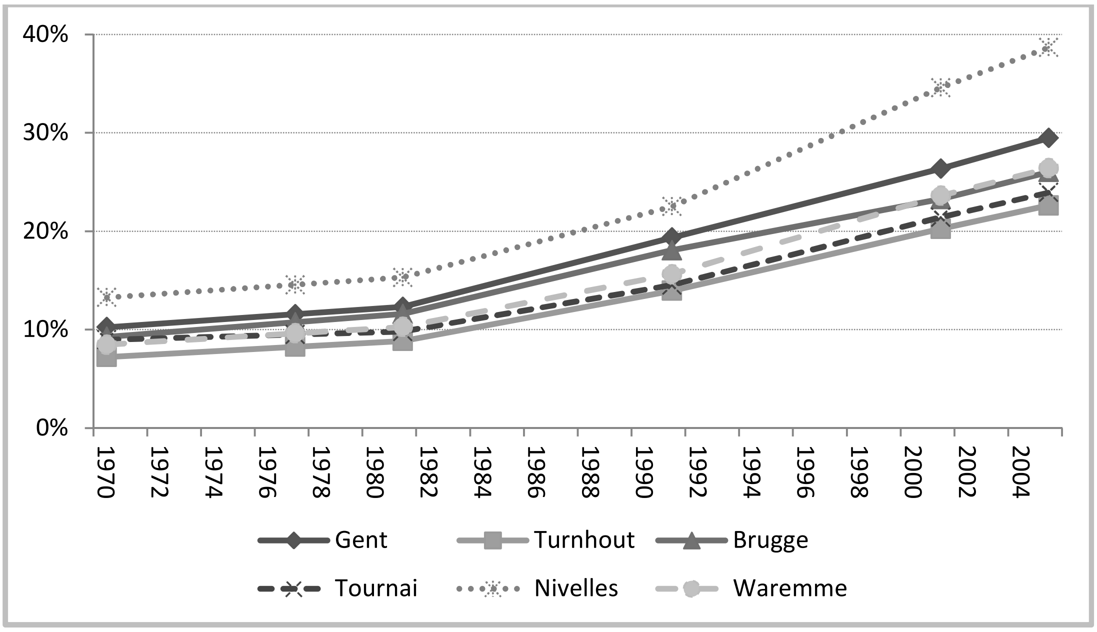
Source: Appendix B.

The wage gap in Figure 5 reflects the evolution of wage costs relative to productivity, i.e. $wage_{it}/q_{it}$. More precisely, it has been computed as the ratio of real compensation per employee (including taxes on labour) to a proxy for real productivity per employee[5]. Our

[5] Due to lack of data at the level of individual districts in the 1970s, our wage and productivity data have been computed at the provincial level. Provinces include about 4 districts on average (see Appendix A).

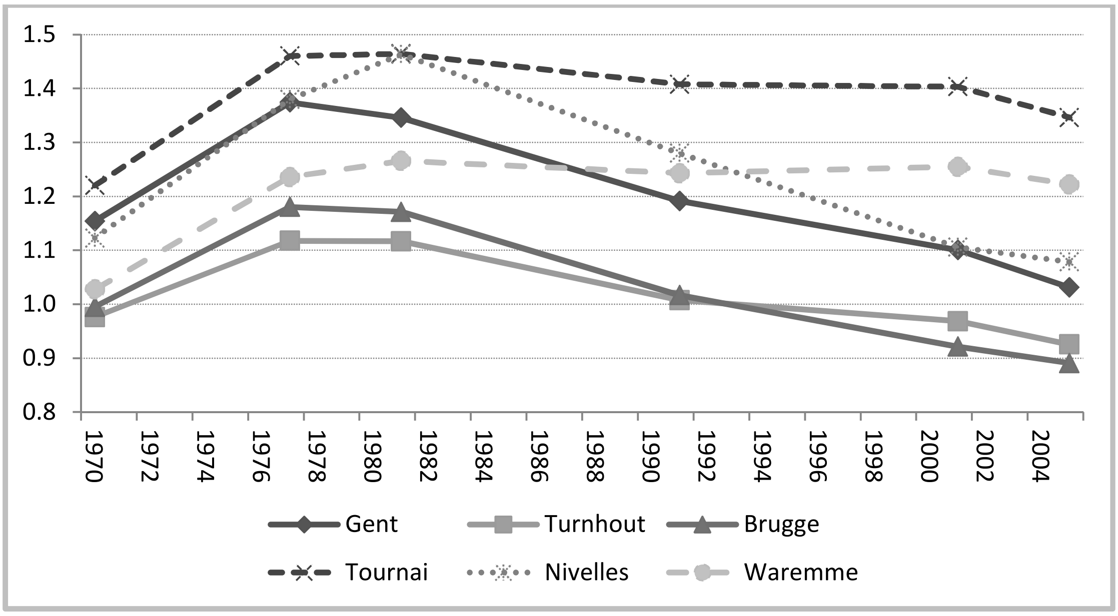
proxy is real GDP per capita. We prefer this variable above output per employee. The latter is highly endogenous, which may disturb appropriate measurement of the wage gap. A simple example can be illuminating. If wage increases are excessive, pushing up the wage gap, firms may respond by laying off the least productive workers and by substituting capital for labour. As a result, output per (remaining) worker may rise, and the wage gap may fall again. In the end, even if there is a serious problem of job losses, the wage gap may reveal nothing. Employing GDP per capita as a productivity measure makes the wage gap much less vulnerable to this perverse mechanism. Our data in Table 2 and Figure 5 are to be interpreted as an index, compared to a benchmark wage gap. As benchmark we chose the wage gap in the whole of Belgium in 1970. The data clearly show a derailment of wage costs in the seventies. During the eighties the wage gap is strongly reduced in Flanders, mainly thanks to higher productivity growth, with comparable wage growth. The wage gap remains much higher in Walloon districts. Wages have not followed (downwards) the weaker evolution of productivity. The data in Table 2 confirm that relative to the within standard deviation, the variation across districts (between standard deviation) is much smaller for the wage level than for productivity. A final series of variables in Table 2 are demographic. We report the share of three age groups in total population: the fraction of people aged 15 to 24, people aged 25 to 54, and people aged 55 to 64.

Figure 4. The percentage of highly skilled people (14-... years old) (Schooling)



Source: Appendix B.

Figure 5. Wage gap (index, Belgium 1970 = 1)



Source: Appendix B.

5. Econometric analysis and results

This section contains our main estimation results. In line with earlier arguments, we estimate Equation (1) using the 2SLS estimation method. Endogenous variables to be instrumented are the rate of ownership, schooling, the wage level and productivity. As a result of the endogeneity of wages and productivity, also the wage gap is endogenous. Since the coefficients γ_3 and γ_4 of the log wage level and log productivity are never significantly different from each other in absolute value, we concentrate in this section on results using the log wage gap[6]. The latter comes down to imposing in Equation (1) the restriction that $\gamma_3 = -\gamma_4$.

5.1 Instrumental variables

Good instruments should have explanatory power for the variable to be instrumented and be uncorrelated to shocks υ_{it} in the employment rate in the individual district.

[6] Estimating γ_3 and γ_4 separately always yields a value for γ_4 close to 0.25 and highly significant, while γ_3 is always negative but very imprecisely estimated (estimated t-value < 1). The value of γ_3 is never significantly different from -0.25.

When it comes to instrumenting real wages and productivity in individual districts, and therefore the wage gap, it is our hypothesis that the 'aggregate' regional counterparts of these variables contain key information on exogenous drivers, without being affected to any important extent by district-specific shocks. For Flemish districts these aggregate regional variables are averages over all 22 districts in Flanders. For Walloon districts we use averages over all 20 districts in Wallonia. Although the cross-sectional variation of these aggregate regional variables is very small, they are fully time-varying. Aggregate real wages for example will reflect changes in wage push variables like union power in key sectors in the region, taxation, and (aggregate regional) labour market policies. Aggregate real wages should not seriously be affected by idiosyncratic labour demand and employment shocks in individual districts, which constitute no more than one twentieth of the regional aggregate. Aggregate variables may of course reflect common shocks across all districts, but due to the use of time dummies these common shocks will not show up in the error term.

Next to the relevant regional aggregate, we include the length of the highway and county road network (in kilometres per km^2) as an additional instrument for productivity in a district, and therefore the wage gap[7]. We call this instrument 'infrastructure'. Highways and main roads being major elements of the infrastructure in an area, the causal link with productivity is obvious.

For schooling in a district we define population density in that district as instrument. We rely on Boucekkine et al. (2007) who have shown that high(er) population density in an area promotes the enlargement of education facilities. More schools being nearby will then open the possibility to reach higher education levels for more people. Highly populated areas may also attract more public transport connecting people to higher education facilities at larger distance

Finally, we specify four instruments for the rate of ownership in a district. A first one is the fraction of the population older than 35 (age35+). The literature has shown the explanatory power of various demographic variables for the rate of ownership (e.g. Rosen and Rosen, 1980; Barrios García and Rodríguez Hernández, 2004; Gwin and Ong, 2008). The fraction of the population older than 35 is expected to have a significant positive effect.

[7] Productivity data being available only at the provincial level (see footnote 4), our data for the road network also concern the province to which a district belongs.

People in this age group have generally more resources and higher preference to enter into long-term commitments than younger people. Our second and our third instrument are population density and its square. Population density acts as a proxy for urbanization. It has been shown in the literature that differences in urbanization contribute significantly to explain variation in the rate of homeownership. The relationship is negative (e.g. Fisher and Jaffe, 2003). The reason for also including squared population density, is to allow for non-linearity in this relationship. Coulson and Fisher (2009) provide one explanation for this negative relationship when they point to the fact that owner-occupied dwellings tend to be single-family detached units, whereas rentals are more often in multifamily dwellings. The latter are much more frequent in urban areas with high population density. Our fourth instrument for the rate of ownership is a common time trend for the six districts to which the major Belgian cities (next to Brussels) belong. These districts are Antwerp, Ghent, Bruges, Charleroi, Liege and Namur. Corresponding cities all house more than 100.000 people. This common time trend captures the differential positive effect on the rate of ownership in the biggest cities and their suburbs from various structural developments since the 1970s. These developments include rising land prices in less urbanized areas, increasing traffic and more frequent traffic jams on the main axes around big cities, rising rental costs in percent of disposable income, a fall in the age at which people buy their first own house combined with the relative preference of young people to live in bigger cities, and government policies (so called ’grootstedenbeleid’ since 1999) raising the attractiveness of living as owner in big cities (Vanneste et al., 2007; Vastmans and Buyst, 2011). These structural developments did not all take place simultaneously, but they contribute to explaining the stronger trend rise in the rate of ownership in the biggest cities and their suburbs over time[8].

To test the quality of our instruments, we first assessed their explanatory power in the first stage regression for the endogenous variable that they are expected to explain. All but one instruments show up statistically significant at 2%. All have the correct sign in these

[8] Note that the structural developments underlying this ‘major city time trend’ bear no relationship to the employment rate. Extending Equation (1) with this time trend yields a highly insignificant coefficient (t-value < 0.7 in absolute value). This conclusion of insignificance holds for all instruments when added to our estimated employment equations. They do not matter for employment significantly beyond their influence on the endogenous variables in the regression.

regressions[9]. Table 3 summarizes for the endogenous real wage gap, schooling, and rate of ownership in a first row the list of their instruments and the corresponding first stage F-statistics. These test the null hypothesis that the instruments do *not* significantly enter the first stage regression. The values for the F-statistic that we obtain are always far above Staiger and Stock (1997)'s rule of thumb value of 10, supporting our instruments' joint significance. A second F-statistic in the 'all six instruments' row is the F-value for the null that all instruments are irrelevant, including those that we selected for other endogenous variables. Again we obtain values above 10. Next to their explanatory power, we tested the instruments' exogeneity. We report overidentification test statistics (J-statistics) for their validity at the bottom of Table 4. We can never reject the null hypothesis that the instruments are valid.

Table 3. Instruments and instrument relevance

Endogenous variable	Set of instrumental variables	First stage F - statistic testing the relevance of the instruments
log real wage gap	regional aggregate log real wage gap infrastructure	21.5
	all six instruments	10.3
schooling	population density	20.9
	all six instruments	27.1
ownership	fraction of population older than 35 (age35+) time trend for districts of 6 major cities population density population density squared	17.5
	all six instruments	13.4

Data sources and summary of descriptive statistics for the instruments: see Appendix B and Table B1.

5.2 Basic estimation results

To estimate our equations we use the fixed effects estimator. A Hausman test rejects the null hypothesis that the unobserved effects are uncorrelated with the explanatory variables. Column (1) in Table 4 contains estimation results for our basic Equation (1), still allowing

[9] Detailed results are available upon request. The exception is the fraction of people older than 35 in the first stage regression for ownership. It is significant at 11%.

Table 4. Estimation results for the employment rate (Equation 1)

EMPLOYMENT	1 – 2SLS	2 – 2SLS	3 – 2SLS	2 - OLS
Homeownership rate (OWN)	-0.285(**) *(0.17)*	-0.358(***) *(0.08)*	-0.327(***) *(0.08)*	-0.257(***) *(0.04)*
Schooling	-0.005 *(0.14)*	0.038 *(0.12)*	0.066 *(0.13)*	0.115 *(0.07)*
100*Log (wage)	-0.114 *(0.22)*	-	-	-
100*Log (productivity)	0.249(***) *(0.08)*	-	-	-
100*Log (wage gap)	-	-0.225(***) *(0.06)*	-0.235(***) *(0.06)*	-0.130(***) *(0.02)*
Fraction age 15-24	-0.220 *(0.20)*	-0.185 *(0.19)*	-	-0.177 *(0.18)*
Fraction age 15-24 X Dummy1970-1989	–	–	-0.132 *(0.19)*	–
Fraction age 15-24 X Dummy1990-2005	–	–	-0.302 *(0.21)*	–
Fraction age 55-64	-0.278(*) *(0.17)*	-0.276 *(0.17)*	-	-0.249 *(0.16)*
Fraction age 55-64 X Dummy1970-1989	–	–	-0.210 *(0.19)*	–
Fraction age 55-64 X Dummy1990-2005	–	–	-0.367 *(0.23)*	–
Dummy Flanders 1990-2005	2.53(***) *(0.72)*	2.37(***) *(0.70)*	2.47(***) *(0.71)*	3.05(***) *(0.43)*
R-squared within	0.87	0.87	0.87	0.88
R-squared between	0.25	0.16	0.18	0.19
R-squared overall	0.43	0.37	0.40	0.44
J-statistic (p-value) [(a)]	0.19	0.20	0.33	-
Time dummies	yes	yes	yes	yes
District dummies	yes	yes	yes	yes
Number of observations	232	232	232	232

Note: * (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors.
(a) Sargan-Hansen J-test of overidentifying restrictions. The null hypothesis is that the overidentifying restrictions are correct.

unrestricted γ_3 and γ_4 [10]. As we have mentioned before, γ_3 emerges highly insignificant. Column (2) introduces the log real wage gap, and therefore imposes the restriction that γ_3=-γ_4. Our estimation results in columns (1) and (2) show significant negative effects from

[10] Instead of the aggregate regional real wage gap, we introduce aggregate regional productivity and aggregate regional wages as two separate instruments (next to infrastructure) to estimate this equation.

the rate of ownership and highly insignificant effects from the fraction of highly educated (schooling). The effects from the real wage gap in column (2) are significantly negative. Furthermore, we find negative effects on the employment rate from the share of young and older people in the population, which confirms expectations, but these negative effects are not (or only weakly) significant. In column (3) we allow variation over time in the effects of these two demographic variables. In line with expectations formulated earlier, we observe that the negative effects are larger in the second part of the period that we study (1990-05) than in the first part (1970-89), but nothing is statistically significant here[11]. Finally, our results reveal a significant positive differential Flemish policy effect on the employment rate of a little more than 2 percentage points since 1990. The far right column (4) in Table 4 re-estimates column (2) by the OLS method.

Our results confirm the Oswald hypothesis. We find in columns (1) – (3) that a 1 percentage point rise in the rate of ownership in a district implies a significant fall in the employment rate in that district by about 0.3 to 0.35 percentage points. This effect is not only statistically significant, it is also important economically. Using the estimated coefficients in column (3) and the data in Table 2, one can compute for example that a one standard deviation rise in the wage gap implies a fall in the employment rate by about 3 percentage points. A one standard deviation rise in the rate of ownership may cause a fall in the employment rate by no less than 2.6 percentage points. These findings underscore the importance of housing and the arguments underlying the Oswald hypothesis for employment in Belgium. In this respect, our results are in line with earlier work by Estevão (2002) and OECD (2011). Investigating regional labour market disparities in Belgium, Estevão finds low labour migration, and concludes that "Belgians move too little". He points at linguistic and cultural factors, a compressed wage structure and generous unemployment benefits to explain low mobility. Although our study is not about mobility, it would suggest a high rate of homeownership as another potential explanatory variable. OECD (2011) confirms the negative effect of homeownership on mobility. This OECD study also indicates Belgium as a country with very high transaction costs of buying and selling houses.

[11] Also including the fraction of prime age workers in the employment equation implied coefficients which were highly insignificant and almost zero for this age group.

Our results also underscore the importance of the estimation method and of controlling for cross-sectional fixed effects and common time effects, when testing the Oswald hypothesis. We observe in the far right column in Table 4 the bias that may follow from OLS estimation. Given the expected positive effect of the employment rate (as a determinant of permanent income) on ownership, it should be no surprise to observe a weaker Oswald effect when we do not control for endogeneity. Its estimated size falls by about 30 percent. Note, however, that this reduced Oswald effect is still significantly different from zero. This result demonstrates that our main conclusion in this paper is invariant to the estimation technique (IV, OLS).

Table 5 contains estimation results where we do not control for cross-sectional fixed effects and/or common time effects. The estimation errors that occur here, are much more serious. As one can see, anything goes. If common time effects are not controlled for in column (2_b), the Oswald coefficient falls to a little more than 1/2 of its estimated value in Table 4[12]. Not controlling for district fixed effects in column (2_c) yields an estimated Oswald coefficient which is even slightly positive, although statistically insignificant. If we control neither for district fixed effects nor for common time effects in column (2_d), a positive and statistically significant coefficient of 0.096 shows up.

Our results in Table 5 may also shed light on the (somewhat surprising) insignificance of schooling in Table 4. One explanation is that (in contrast to other variables) schooling shows a highly similar evolution over time in all districts. Even if this evolution is important for employment, its effects may at least partly be picked up by the common time dummies[13]. Dropping these time dummies in Table 5, but controlling for district fixed effects, yields a positive and highly significant schooling effect (see column 2_b).

[12] Imagine for example business cycle shocks. Positive shocks may raise both employment, aggregate wages, and household confidence and resources, and the ambition to become owner. If not controlled for in the regressions, such a shock will induce positive correlation between ownership and the error term, and bias upwards the estimated Oswald coefficient.

[13] The estimated time dummies in Table 4, column (2), are respectively 4.3%, 3.3%, 1.9%, 5.0% en 7.3% in 1977, 1981, 1991, 2001 and 2005.

Table 5. Additional estimation results for the employment rate

EMPLOYMENT	2_b - 2SLS	2_c – 2SLS	2_d – 2SLS
Homeownership rate (OWN)	-0.188(**) *(0.09)*	0.045 *(0.07)*	0.096(*) *(0.05)*
Schooling	0.246(***) *(0.06)*	-0.823(*) *(0.44)*	0.078 *(0.08)*
100*Log (wage gap)	-0.146(***) *(0.03)*	-0.022 *(0.07)*	-0.191(***) *(0.02)*
Fraction age 15-24	-0.148 *(0.26)*	0.705(***) *(0.17)*	0.650(***) *(0.13)*
Fraction age 55-64	-0.296 *(0.24)*	0.443 *(0.29)*	0.239 *(0.22)*
Dummy Flanders 1990-2005	2.17(***) *(0.65)*	4.55(***) *(1.53)*	1.59 *(1.00)*
R-squared within R-squared between R-squared overall	0.74 0.23 0.43	- - 0.40	- - 0.62
J-statistic (p-value) (a)	0.00	0.98	0.00
Time dummies District dummies	no yes	yes no	no no
Number of observations	232	232	232

Note: * (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors

(a) Sargan-Hansen J-test of overidentifying restrictions. The null hypothesis is that the overidentifying restrictions are correct.

5.3 Additional results: size of the Oswald effect, robustness

Table 6 summarizes the results of a series of additional regressions that we have run, and where we include not just OWN_{it} in the employment regression, but also one or more interaction terms $OWN_{it}*VAR_i$, where VAR_i stands for a structural variable at the district level which may affect the size of the Oswald effect. Included structural variables are: a dummy for districts situated at a national or linguistic border[14], a dummy for districts close to one of the major cores of economic activity in Belgium (Brussels, Antwerp, Ghent, Liege and Charleroi), the log of average population density in the district, and the log of the average share of highly educated inhabitants[15]. Another interaction term that we include is a dummy common to all districts for the more recent period 1990-2005. Including this dummy (times OWN_{it}) allows to test whether the Oswald coefficient has changed over time.

[14] This also includes districts at the border between Dutch-speaking Flanders and French-speaking Wallonia.

[15] These averages are computed per district over all years in our sample (1970, 1977, 1981,…).

Table 6. Influence of structural variables on the estimated Oswald coefficient

Determinants of the Oswald effect. **Different effect...**	**Change in estimated Oswald coefficient [a]**	
	Interaction term included separately	Interaction terms included together (and *p*-value ≤ 30%)
for districts at a national or linguistic border? District where at least 30% of the municipalities are situated at a national or linguistic border (versus other districts)	- 0.383 (°)	-
for districts close to an economic centre / major city (b)**?** Districts close to an economic centre (versus other districts)	+ 0.222 (°)	-
for densely populated areas? Effect of a rise in population density by one standard deviation (= +226 persons per square kilometre) (c)	+ 0.323	-
depending on the share of highly educated people ? Effect of a rise in the fraction of highly educated by one standard deviation (= +2.45 percentage points in 'schooling') (c)	+ 0.132 (°)	+ 0.160(*)
in the past versus more recent periods? Change in estimated Oswald coefficient for 1990-2005 (versus 1970-1989)	+ 0.144 (°)	- 0.047(°)

Note: (*) (°) statistically significant at less than 10% (20%).
(a) A negative change points at a stronger Oswald effect.
(b) Interaction term is a dummy that equals 1 in the districts of the major cores of economic activity (that is, Brussels, Antwerp, Ghent, Liege and Charleroi) and in the adjacent districts.
(c) Standard deviations are determined over the 42 district averages for population density/schooling over 1970-2005. The log of these district averages are also the data for VAR_i that we use in the interaction term $OWN_{it}*VAR_i$ by which we extend Equation (1).

The data in Table 6 indicate the change in the estimated effect from the rate of ownership on the employment rate brought about by the interaction variable. One column shows the results from including each interaction term separately to the regression reported in Table 4, column (2). The other column follows from including all interaction terms together but dropping those with *p*-values above 30%. Only two interaction terms survive here. Only one of these is statistically significant at 10%. Our results reveal a weaker Oswald effect in districts with a higher share of highly educated people, thereby confirming Dohmen (2005). As to other interaction terms, we see a stronger Oswald effect in districts closer to a linguistic or country border. All other things equal, proximity of a border may imply higher costs to be mobile (e.g. personal costs due to a change of language, or transaction costs due to a shift of legal regime). Furthermore, the Oswald effect would seem to be weaker in densely populated districts and in districts closer to major cores of economic activity (i.e.

districts close the major cities). However, none of these differences are statistically significant. Neither do we observe significant differences in the Oswald effect over time. Additional tests with different time periods than those reported in Table 6 did not yield any interesting results.

Table 7 includes the main results of a number of robustness checks on our findings in column (2) in Table 4. In particular we tested the robustness of the estimated coefficient on ownership (γ_1) for changes in the functional form that we impose on the relationship between ownership and the employment rate, and for changes in the included years.

Table 7. Robustness checks to the regression result in column (2) in Table 4

Robustness checks: estimated coefficients in case we...	γ_1
Include log(OWN) instead of OWN as explanatory variable [(a)]	-0.189(***) *(0.04)*
Include log(Empl) instead of Empl as dependent variable [(b)]	-0.667(***) *(0.14)*
Compute the employment rate as the ratio of the number of jobs in a district to population at working age	-0.345(**) *(0.15)*
Drop the year 1977 (for which many data were missing and had to be computed by interpolation, see Appendix 2)	-0.351(***) *(0.09)*
Drop the year 2005 (for which ownership data were missing for Walloon districts, and some other sources had to be explored for other variables, see Appendix 2).	-0.469(***) *(0.10)*
Drop the years 1977 and 2005	-0.461(***) *(0.11)*
Replace the employment rate as dependent variable by the unemployment rate	0.302(***) *(0.11)*

Note: (**), (***) statistically significant at less than 5% (1%).

(a) The Oswald effect as we report it in this paper (i.e. dEmpl/dOWN) can be derived as the estimated coefficient (-0.189) divided by the level of OWN. Evaluated at the overall sample mean (70%), this implies an Oswald effect equal to -0.27.

(b) The Oswald effect as we report it in this paper (i.e. dEmpl/dOWN) can be derived as the estimated coefficient multiplied by the level of Empl. Evaluated at the overall sample mean (57%), this implies an Oswald effect equal to -0.38.

Furthermore, in one regression we introduced the number of jobs located in a district as the dependent variable, rather than the employment rate among the people living there. None of these changes have important effects on our results. The last row of Table 7 shows the estimated Oswald effect when we introduce a somewhat more fundamental change. Here we estimate our model with the unemployment rate as dependent variable. A first reason for introducing this change is that Oswald's thesis mainly concerns the unemployment rate. A second one is that movements in homeownership may also induce changes in labour force participation, implying a difference between the response of unemployment versus that of employment. For example, due to a positive wealth effect, homeowners may retire earlier than renters. Also, the need for both man and wife to work may be smaller when they are outright homeowners. We show detailed estimation results with the unemployment rate as dependent variable in Appendix C. The results are highly similar to those explaining the employment rate. The estimated Oswald coefficient is 0.30, and statistically very significant. Again we observe a strong fall in this coefficient when we disregard endogeneity and estimate by means of OLS.

6. Conclusions

In a number of papers A.J. Oswald argues that high rates of homeownership may imply inferior labour market outcomes, both for the individual and in the aggregate. This paper tests Oswald's hypothesis in a macro panel of 42 Belgian districts since the 1970s. The use of data going back to 1970 allows us to embed the Oswald hypothesis in a broader model including important other determinants of employment like labour costs and productivity, the skill level of the population, and a number of demographic variables. Considering that ownership may be endogenous to (shocks in) employment, we mainly use IV estimation methods.

Overall, we find evidence in favour of Oswald's hypothesis. We observe that a 1 percentage point rise in the rate of homeownership in a district implies a statistically significant fall in the employment rate by about 0.35 percentage points. The size of this effect is economically important. Additional estimation reveals that the Oswald effect is smaller in districts with higher fractions of high skilled. Our results underscore the importance of controlling for unobserved cross-sectional fixed effects and common time effects, and of appropriately dealing with endogeneity. Disregarding one or more of these issues, as is generally done in the macro labour literature, may imply very different estimation results. (We then observe a weaker Oswald effect, or no Oswald effect at all). Our main result in favour of Oswald's hypothesis survives various robustness checks. These include changes in the dependent variable. Changing our focus to unemployment rather than employment, does not affect our main conclusion.

The literature on the effect of homeownership on employment shows a remarkable contradiction. Micro studies generally reject Oswald's hypothesis, whereas most macro studies support it. One explanation for this contradiction may the methodological weakness of many macro studies. We avoid these weaknesses in this paper but still confirm Oswald's hypothesis, at least for Belgium. This leaves a very interesting avenue for further research, where we shall use micro data for Belgium to estimate the influence of housing status on unemployment duration, like in Munch et al. (2006) and Battu et al. (2008). Is Belgium different, and does the adverse employment effect from homeownership in macro data also exist at the individual level? Factors that might make Belgium different can for example be very high transaction costs of buying and selling houses, relatively generous unemployment benefits (long benefit duration), or the specific linguistic situation restraining mobility. Or, alternatively, is the adverse macro relationship rather due to negative effects from (high) ownership beyond the owners themselves, like traffic congestion, or a tightening of rental markets?

Appendix A: Belgian provinces and districts

Figure A1. Belgian provinces and districts

Average size of a district: 723 km²

Average number of inhabitants per district: 214,000

Appendix B: Data description and sources

Most our data have been taken from the national censuses held in Belgium. Because we only had censuses in the years 1970, 1981 and 1991, we had to supplement these with data from extensive surveys (Social-Economic Survey, Woonsurvey). These surveys have been tested and confirmed to be representative by the statistical authorities (Nationaal Instituut voor de Statistiek, NIS). Not every variable is available in the censuses and surveys, so for some data we had to rely on other sources. We now consider every variable of our model and give a short description. We mention data sources and possible data shortages or adjustments. Table B1 contains the main descriptive statistics for those variables (instruments) that are not yet included in Table 2.

OWN (Homeownership rate)

The fraction of houses that are occupied by their owner.

Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 1977 and 2001 by NIS; Woonsurvey 2005 by the Flemish Government. Data for 2005 are not available for the Walloon districts.

Empl (Labour market performance, employment rate)

Employment rate among households living in the district, i.e. the number of people with a job in the district in percent of the population at working age. Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 2001 by NIS; 'Steunpunt Werk en Sociale Economie' for 2005. The data for 1977 have been interpolated from the years 1970 and 1981.

Unemployment (Labour market performance, unemployment rate)

Unemployment rate among households living in the district, i.e. the number of people without a job but seeking employment in percent of the labour force. Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 2001 by NIS; 'Steunpunt Werk en Sociale Economie' for 2005. The data for 1977 have been interpolated from the years 1970 and 1981.

Schooling

The number of highly skilled people (tertiary education) in percent of the population of age 14 or older. Source: Census 1970, 1981 and 1991; Eurostat for 2001. 1977 has been interpolated from 1970 and 1981. 2005 has been extrapolated from 2001 based on the data of 2001 and the evolution of the national mean according to NIS Labour Force Survey.

Wage

Real compensation per employee. Source: own calculations based on Cambridge Econometrics data. Data for 1970 and 1977 have been extrapolated based on NIS Social Statistics. Due to data limitations, wages have been computed at the level of the provinces. Provinces include about 4 districts on average.

Productivity (q)

Real GDP per capita. Source: own calculations based on Cambridge Econometrics data. Per capita GDP in 1970 and 1977 have been extrapolated based on Cambridge Econometrics and OECD Economic Outlook data. Due to data limitations, productivity has been computed at the level of the provinces. Provinces include about 4 districts on average.

Wage gap

Ratio of wage level to productivity, index with Belgium in 1970 = 1.

Demographic variables

- **Age xx-yy**: People of age between xx and yy in percent of the total population. Source: Census 1970, 1981 and 1991 by NIS; Social-Economic Survey 2001 by NIS for 2001, and 'FOD Economie, KMO, Middenstand en Energie' for 2005 (Rijksregister). The data for 1977 have been interpolated from the years 1970 and 1981.

Population Density

Population density, number of people per square kilometre.

Sources: district area: Eurostat; district population: Census 1970, 1981 and 1991, Social-Economic Survey 2001, 1977 has been interpolated from 1970 and 1981. 2005: Ecodata, 'FOD Economie, KMO, Middenstand en Energie' (Rijksregister).

Infrastructure

The length of highways and county roads per square kilometre.

Source: 1970, 1977, 1981 and 1991: NIS, Statistical yearbooks for Belgium; 2001 and 2005: FOD Mobiliteit en Vervoer (Processing: FOD Economie (Afdeling Statistiek)).

Table B1. Main descriptive statistics for the instrumental variables

	Infrastructure	Population density	Age 35+
Overall Mean	0.443	333	52.0%
Minimum	0.249	34	41.1%
First quartile	0.390	154	49.6%
Median	0.464	268	52.2%
Third quartile	0.502	506	54.6%
Maximum	0.544	955	59.3%
Std. Dev.	0.074	226	3.5%
Between Std. Dev.	0.034	228	1.7%
Within Std. Dev.	0.066	16	1.8%
1970 mean	0.350	321	50.4%
1977 mean	0.392	326	50.6%
1981 mean	0.418	328	50.6%
1991 mean	0.479	333	52.2%
2001 mean	0.510	342	54.3%
2005 mean	0.512	347	53.9%
Observations	252	252	252

Appendix C: Estimation results for the unemployment rate

Table C1: Estimation results for the unemployment rate

UNEMPLOYMENT	**2 – 2SLS**	**3 – 2SLS**	**2 - OLS**
Homeownership rate (OWN)	0.302(***) *(0.11)*	0.299(***) *(0.11)*	0.118 (**) *(0.05)*
Schooling	0.004 *(0.18)*	0.005 *(0.17)*	-0.119 *(0.09)*
100*Log (wage gap)	0.363(***) *(0.08)*	0.362(***) *(0.08)*	0.129(***) *(0.03)*
Fraction age 15-24	0.004 *(0.25)*	-	-0.013 *(0.21)*
Fraction age 15-24 X Dummy1970-1989	–	-0.013 *(0.26)*	–
Fraction age 15-24 X Dummy1990-2005	–	-0.010 *(0.29)*	–
Fraction age 55-64	0.363 *(0.24)*	-	0.302 *(0.19)*
Fraction age 55-64 X Dummy1970-1989	–	0.329 *(0.25)*	–
Fraction age 55-64 X Dummy1990-2005	–	0.445 *(0.31)*	–
Dummy Flanders 1990-2005	-4.00(***) *(1.01)*	-3.95(***) *(1.01)*	-5.87(***) *(0.51)*
R-squared within	0.84	0.84	0.89
R-squared between	0.16	0.16	0.25
R-squared overall	0.44	0.44	0.66
J-statistic (p-value) [(a)]	0.88	0.89	-
Time dummies	yes	yes	yes
District dummies	yes	yes	yes
Number of observations	232	232	232

Note: The estimation results in this table correspond to those in Table 4, but have the unemployment rate as dependent variable. The set of instruments used in the 2SLS regressions includes the aggregate regional log real wage gap, population density, population density squared, a time trend for the districts of the 6 major cities, and the fraction of the population older than 35.
* (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors.
(a) Sargan-Hansen J-test of overidentifying restrictions. The null hypothesis is that the overidentifying restrictions are correct.

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CHAPTER 3

Housing tenure and geographical mobility in Belgium

Housing tenure and geographical mobility in Belgium

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Abstract

Housing tenure is a key determinant of geographical mobility. We estimate several probit models to explain the probability that households move, using Belgian longitudinal PSBH and EU-SILC datasets which together cover the period 1994-2009. We confirm the general conclusion in previous literature, that homeowners are, ceteris paribus, less mobile than tenants. Within the first category, having a mortgage further hampers mobility. Earlier results for Belgium did not find a significant difference between outright owners and mortgagees. Furthermore, we make progress on the existing literature by paying particular attention to (and dealing with) methodological issues such as unobserved heterogeneity and state dependence. However, we also obtain some indications that the strict exogeneity assumption may be violated, implying that we cannot exclude the possibility of some bias in our estimated coefficients.

JEL classification: J61, R21, R23

Keywords: Housing tenure, geographical mobility, Belgian households, panel data

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1. Introduction

The most cited study in the literature regarding residential mobility, is assumably the work of Rossi (1955). This book is believed to have had the greatest influence on later empirical work since it was the first to extensively analyze geographical mobility at the micro level. Before, academic research mainly concentrated on aggregate mobility flows between regions. Rossi (1955) already concluded that housing tenure is one of the major determinants of mobility. Although he conducted his research from a primarily sociological point of view, his micro focus was rapidly adopted by economists. Many studies elaborated further on this early work, often focusing on diverse subfields (e.g. housing satisfaction by Diaz-Serrano & Stoyanova (2010); quality of the neighbourhood by Rabe and Taylor (2010a); public policy implications by Caldera Sánchez and Andrews (2011b); social capital by Kan (2007) and house prices and housing supply by Ferreira et al. (2008) and Rabe and Taylor (2010b)).

In economics, a major motivation for analyzing residential mobility is assuredly the link between geographical mobility and the labour market. As Blanchard and Katz (1992) demonstrate, mobility is a key instrument to resolve mismatches in the labour market. Mobile workers will move from regions hit by adverse labour demand shocks to regions with a higher labour demand or with specific requests for specialized skills. In the early research by Rossi (1955) and later by Speare et al. (1975), Hughes and McCormick (1981, 1987) and Clark and Dieleman (1996), the consensus holds that tenants are more mobile than homeowners. The latter experience higher costs when buying and selling a dwelling[1]. The research topic gained increasing attention since the influential work of Oswald (1996, 1997), in which these arguments where used to explain the harmful effects of homeownership on labour market outcomes. In particular, Oswald concludes that countries/states with a higher fraction of homeownership suffer from higher unemployment rates. In this paper we investigate whether owners are indeed less geographically mobile in Belgium. Of course, finding lower mobility would not directly prove Oswald's theory to be valid. It should be rather considered as a necessity and not a sufficiency for the validity of Oswald's hypothesis.

[1] These might include search costs, all kinds of transfer taxes, real estate agency fees, solicitor fees and refinancing costs.

The existing empirical literature can be categorized along several axes. A first one is the type of dataset that is used. First, a few studies (e.g. Gardner et al. (2001), Ermisch and Di Salvo (1996)) were able to use data that run over a very long period, even multiple decades. This enables the researchers to observe individual's complete moving history and therefore adopt a duration analysis. Although it is a powerful tool, important drawbacks are the high rate of attrition and the loss of quality when retrospective data are collected with large intervals. Second, many studies use cross-sectional data (e.g. Hughes & McCormick (1981, 1987), Helderman et al. (2004), Caldera Sánchez & Andrews (2011b)). The main shortcoming of these studies is that they fail to account for unobserved heterogeneity. Third, longitudinal panel data that cover a limited time period became increasingly popular over the last few decades. The advantages over the cross-sectional datasets are clear. The main drawback is that they are only able to analyze a snapshot of the total moving history. In the empirical section, we explain how we attempt to resolve this matter.

A second way to categorize is in the distinction between different types of housing tenure that have been introduced by researchers, and in the differential effects each type may have. First, tenants can be alternatively defined as private tenants or social tenants. Hughes and McCormick (1981, 1987) find that the higher mobility of social tenants compared to owners only holds over short distances. When only considering moves over a long distance, this group is even less mobile than owners. The reason for this is that public sector tenants could risk losing their benefits if they migrate. Often, the demand for social accommodation is higher than the supply, so people could end up on waiting lists or lose their chance of public housing entirely (Champion et al., 1998). Hence, the group of tenants cannot be considered as a homogeneous group. Second, a more recent distinction has been made between homeownership with a mortgage and outright homeownership. There is much less of a consensus about this topic. From a theoretical point of view, Böheim and Taylor (2002) address negative equity as a potential obstruction to mobility, in particular for mortgagees. If house prices drop, homeowners are restrained to move since a transaction will transpose their virtual loss into actual loss. Although Belgian average house prices did not decrease since the first half of the 1980s, the negative equity effect can still emerge for a different reason. According to Catte et al. (2004) and Van Ommeren and van Leuvensteijn (2005), the above described housing transaction costs are about 23% of the total house price in Belgium, directly causing a lock-in effect for homeowners[2]. We deduce two reasons why mortgage holders are constrained the most in this framework. A first one is that transaction costs are higher due to bank costs and extra solicitor fees. As a second one, mortgage holders are assumed to be more prone to the negative equity trap than outright owners, due to the financial constraints in case debt exceeds the market value of their current residence. Caldera Sánchez and Andrews (2011a) raise one counterargument to this: the mortgagees' greater incentive to remain employed or regain employment more rapidly. According to their theory, owners with a mortgage will have a larger urge for mobility to preserve the ability to repay their mortgage.

Empirically, the panel data analyses of Böheim and Taylor (2002) and Rabe and Taylor (2010a) conclude that outright owners are significantly more mobile than owners with a mortgage. However, Caldera Sánchez and Andrews (2011a,b) find the opposite result in almost all OECD countries, using a cross-section of EU-SILC data. They confirm the lower mobility of mortgagees only for Israel, Luxemburg, Norway, the UK and the US. Belgium is one of the exceptions where no significant difference between the two groups of homeowners is found.

In this paper, we investigate the impact of housing tenure on residential mobility using the PSBH and more recent EU-SILC datasets. We control for differences between both types of tenants and further investigate the disputed effect of having a mortgage for owner-occupiers. We derive the model specification from the abovementioned literature and adopt the methodology that has been employed by Tatsiramos (2009), Rabe and Taylor (2010a) and Diaz-Serrano and Stoyanova (2010). We handle unobserved heterogeneity and contribute to these models by treating the initial conditions problem. To our knowledge, this is the first extensive study for Belgium. Only Caldera Sánchez and Andrews (2011b) report estimates for Belgium within an international comparison, based on cross-sectional data. For various reasons, Belgium is a very interesting case to study. Since it is a very densely populated country, the social costs of commuting are considerable. Therefore, a high degree of residential mobility is desirable. Also, the number of homeowners in Belgium is very high compared to most Western countries, representing about 70% of Belgian households

[2] Since 2002, Flanders altered its system of stamp duties, permitting a discount in stamp duties subject to the stamp duties paid for a previous purchase. This policy measure mitigates the lock-in effect to some extent.

according to our sample. If homeowners are indeed less mobile, this may have a very large impact on the Belgian labour market. Finally, the above mentioned Belgian transaction costs are the highest among OECD countries. Consequently, residential mobility of homeowners, especially with a mortgage, is expected to be very low. The paper confirms that homeowners are indeed less mobile than tenants. If the owner-occupier has a mortgage, mobility further decreases. Social tenants have a lower propensity of moving than private tenants. Although we tackle a number of methodological issues by employing more advanced estimators, caution is still required when interpreting the results because of a potentially remaining endogeneity bias in the estimates.

The paper is organized as follows. Section 2 presents the methodological background of the different estimators that we use. In Section 3 we discuss the different determinants that are included in the model specification. Furthermore, some basic descriptive statistics are shown of both the explanatory variables and the dependent variable, geographical mobility, in order to provide the reader with some first insights. In Section 4 we present the estimation results. Section 5 concludes.

2. Methodology

Both data sources, PSBH and EU-SILC, offer the advantage of longitudinal data. First of all, this allows us to analyse expressed rather than revealed preferences of mobility. Second, panel data make it possible to take unobserved heterogeneity into account. Recent studies using only cross-sectional data such as Calderea Sánchez and Andrews (2011b) and Lux and Sunega (2012) explicitly acknowledge it as a shortcoming when a time dimension is lacking. In this section, we elaborate on the different econometric models that are used by Tatsiramos (2009), Rabe and Taylor (2010a) and Diaz-Serrano and Stoyanova (2010). We draw particular attention to the issues of unobserved heterogeneity. We make progress on these studies by also taking state-dependence and the strict exogeneity assumption into account.

2.1 The pooled Probit model

Whether a household moves or not over the subsequent year, is a binary choice. The observed variable y_{it} is equal to 1 in case of mobility between t and t+1, and 0 otherwise. To indicate the panel structure of our data, the subscripts i and t are used to denote the household and year respectively. The probability to move is assumed to depend on a continuous unobserved latent variable y_{it}^* (with $y_{it} = 1$ if $y_{it}^* > 0$ and $y_{it} = 0$ otherwise). We adopt a Probit model. This specifies the probability that $y_{it}^* > 0$ is a cumulative standard normal distribution. The latent propensity y_{it}^* can then be written as:

$$y_{it}^* = \beta_0 + \beta_H H_{it}^{'} + \beta_Z Z_{it}^{'} + \beta_R R_{it}^{'} + v_{it}, \tag{1}$$

with $H_{it}^{'}$: vector of housing tenure dummies

$Z_{it}^{'}$: vector of household characteristics

$R_{it}^{'}$: vector of area characteristics

v_{it}: error term

The exact elements out of which the different vectors consist, are described in the data section. Equation (1) shows a model which ignores unobserved heterogeneity. This is known as the Pooled Probit model and is very similar to the cross-sectional Probit model.

2.2 Dealing with unobserved time-invariant heterogeneity

Having repeated observations for each household enables us to deal with unobserved time-invariant heterogeneity. We decompose the error term from equation (1) into:

$$v_{it} = \mu_i + \varepsilon_{it}, \tag{2}$$

with μ_i denoting the time-invariant term and ε_{it} denoting the time-variant term. Several estimation options are possible to treat unobserved heterogeneity. If the time-invariant household effect μ_i is independent from the explanatory variables, the *Random Effects Probit estimator* is suitable and efficient. However, many scenarios can be thought of in which this assumption is violated. Some unobserved household-specific characteristics may affect both mobility and the explanatory variables. For example, households that have an intrinsically larger urge for stability, will have a lower probability of moving and might also have a higher propensity of homeownership. When these suspicions are justified, the Random Effects estimator produces inconsistent results. In this case, many refer naturally to the Fixed Effects estimator as a valid alternative. However, unlike linear models, non-linear models do not allow to estimate the fixed effects as dummies along with the rest of the equation through MLE when T does not go to infinity. This problem yields inconsistent estimates and is commonly known as the incidental parameters problem (see e.g. Wooldridge, 2010). To overcome this obstacle, it is common to use the Conditional Logit model (Chamberlain, 1980). Diaz-Serrano and Stoyanova (2010) see three important drawbacks to this method. First, observations with only positive or only negative outcomes of mobility are excluded from the sample. This is critical because non-movers in our dataset are a numerous group and particularly relevant to our research question. Second, the effect of time-invariant explanatory variables cannot be estimated. Third, explanatory variables with limited variation over time are weakly estimated because much of the variation is absorbed by the fixed effects. This is an unattractive feature since housing tenure varies rather little over time. Therefore, a more appropriate model is required.

In an attempt to preserve the convenient properties of the Random Effects model without conceding to the rather unrealistic assumption of the covariates being uncorrelated with the error term, Tatsiramos (2009), Rabe and Taylor (2010a) and Diaz-Serrano and Stoyanova (2010) suggest using an alternative specification. Following Mundlak (1978), the assumption is relaxed by allowing that μ_i is indeed correlated. The *Mundlak approach* assumes that the regression function of μ_i is linear in the within-individual mean values of the time-variant explanatory variables (denoted by $\bar{H}_i'$ and $\bar{R}_i'$). For our model, this boils down to:

$$\mu_i = \alpha_0 + \alpha_H \bar{H}_i' + \alpha_R \bar{R}_i' + e_i \text{,} \tag{3}$$

where e_i is the individual effect with $e_i \sim N(0, \sigma_e^2)$ and not correlated with $H_{it}', Z_{it}', R_{it}'$ and v_{it}. α_0 is absorbed by the constant term β_0 in equation (1). Intuitively, this approach can easily be rationalized. If some unobserved fixed household-specific characteristic affects

H'_{it}, Z'_{it} or R'_{it}, then it will be reflected in the individual-specific mean values of these variables, denoted by $\bar{H}'_i$, $\bar{Z}'_i$ and $\bar{R}'_i$. By including these individual-specific mean values in the regression, any relevant influence of the underlying unobserved fixed characteristic is taken out of the error term. Correlation between included explanatory variables and the error term is then no longer possible, at least not for this reason[3]. Note that when we adopt this procedure in Section 4, the vector of within-means of household characteristics $\bar{Z}'_i$ is not included among the Mundlak terms. The reason is that we keep these characteristics constant at their initial value in the first period of observation (see Section 2.4. below).

2.3 State-dependence and the Wooldridge approach

Yet another issue is the possible appearance of state-dependence. In the introduction, we discussed the dichotomy between studies using duration models and research based on shorter longitudinal intervals, using discrete choice models. The latter attempts to evaluate a flow sample using a static model, also known as stock sampling. Many households probably have rarely or never moved while others have moved several times before the start of the survey. This information is ignored in the previously described specifications and is apparently not a point at issue in the aforementioned literature. Past mobility however may influence the probability of current mobility. One might be less tied to the neighbourhood or the house. Also, moving requires a large effort with respect to search and logistic costs. We believe that repeated moves lower these costs through learning effects. Hence, households that moved more recently may be more likely to move in a later period compared to an otherwise identical household. This situation is called state-dependence. Because the period of observation does not coincide with the whole mobility history of the household, the initial condition problem has to be considered. A bias could result from the possible correlation between unobserved heterogeneity and lagged values of the dependent variable. In the aforementioned literature this issue has not yet been tackled.

[3] The effect of unobserved fixed characteristics that are not correlated with the explanatory variables cannot be removed from the error term by this procedure. But that is not a problem. When there is no correlation with the included explanatory variables, the error term will not be correlated with these variables either.

Wooldridge (2005) suggests a computational inexpensive solution to this problem. The *Wooldridge approach* adds the initial condition y_{i0} as a supplementary regressor to the model. Alternatively, we use the number of years that passed (T_0) since the last move at the start of the observed period, as a proxy for the initial condition. This variable controls for the mobility prior to the observed period. Next, we add a yearly changing counterpart (T_t) which captures the duration effect throughout the observed period. The revised specification of the unobserved heterogeneity is denoted by[4]:

$$\mu_i = \alpha_0 + \alpha_H \bar{H}_i' + \alpha_R \bar{R}_i' + \alpha_{T0} T_0 + \alpha_{Tt} T_t + e_i , \qquad (4)$$

2.4 The strict exogeneity assumption

One of the requirements for using a panel model with unobserved heterogeneity is strict exogeneity. This issue has been generally neglected in the empirical mobility literature. Specifically, the error term ε_{it} must be uncorrelated with the explanatory variables in all time periods. A well-known example to clarify the issue is family expansion. The household may move to a more suitable residence because it has fertility plans. This may clearly bias the estimated coefficients. To overcome this issue, we keep most of the variables constant at the initial value in the first period of the observation. In particular we do this for the large vector of household characteristics Z_{it}'. The vector of area characteristics R_{it}' only contains variables at the aggregate level, so for these variables strict exogeneity is assured by definition. We do not adopt this procedure for the housing tenure dummies H_{it}'. The reason is that, when values are fixed to the first period, it is no longer possible to structurally interpret the coefficients. So, for these housing variables we cannot exclude the possibility of some endogeneity. We will test whether or not the strict exogeneity assumption is violated by including the lead variable of housing tenure in the regression. A second drawback of fixing variables at the initial value, is unavoidably that some of the information in our dataset is neglected. If for example the household composition changes during the observed period, this will not be taken into account when analysing the effect on mobility. In this scenario, an omitted variable bias can occur. These two disadvantages probably explain why the previous

[4] Again using a Random Effects estimator.

literature did not adopt this approach. Nevertheless, we believe that neglecting the apparent absence of strict exogeneity for many of the variables, may induce even stronger limitations.

After having discussed the data in Section 3, Section 4 provides the results and evaluates the four models that are introduced in this section. To sum up, the Pooled Probit model neglects unobserved heterogeneity, while the Random Effects Probit estimator requires that it is uncorrelated with the regressors. This assumption is weakened by the Mundlak approach. The Wooldridge approach tackles the potential initial condition problem. Last, we explicitly test whether the assumption of strict exogeneity holds.

3. Data

We use data from two different surveys of Belgian households. Together they cover the period 1994-2009 with a temporary break in 2003. For the period 1994-2002 we use the PSBH survey (Panel Study for Belgian Households). This survey ran from 1992 to 2002. However, since it was only normalized and integrated into the European Community Household Panel (ECHP) in 1994, the first two waves are withdrawn from our panel. The PSBH contains a wide range of socio-economic variables, both at the household level and at the level of its members[5]. It was built to serve as a longitudinal database, making it possible to analyse various social issues over time. From the 9 waves of the panel we can extract 8 time periods (T=8) and a total of 18,262 household-years after omitting the observations with missing values.

Second, we study the period 2004-2009 with the EU Statistics on Income and Living Conditions (EU-SILC), which is the more widespread successor of ECHP[6]. It comprises similar variables as its predecessor, although a direct comparison between both is not appropriate. The main difference is the set-up of the dataset. The EU-SILC dataset is also longitudinal but it is constructed as a rotating panel of 4 subsamples, each year replacing one fourth of the total sample. In this way, a household can stay in the panel for a maximum of 4 years. Each

[5] More information and metadata: http://www.psbh.be

[6] More information and metadata: http://epp.eurostat.ec.europa.eu/portal/page/portal/microdata/eu_silc

subsample contains initially 1,500 households. Our total panel counts 6 waves (so T=5) and a total of 13,434 household-years.

A first choice in constructing a feasible dataset is whether to work with households or individuals as sample units. To resolve this issue, we follow the arguments of Hughes and McCormick (1981) and van Ham et al. (2010). The event of a residential move is a household decision and the probability of moving depends to a large extent on household characteristics. Alternatively, Hughes and McCormick (1987) use the “heads of households” as sample unit, but this might seem somewhat arbitrary. Of course, sometimes households are forced to move when a split occurs as a result of a divorce or the end of co-housing. We consider these events as random and not in the scope of our subject. Therefore, these households are eliminated from the sample.

A second refinement of our dataset is to drop households without at least one person of the age group 25 to 64. The lower boundary (age 0-24) largely filters out first time movers and students while the upper boundary (age 65-…) filters out the so-called pension mobility. As noticed in the introduction, we are particularly interested in mobility benefiting the labour market and therefore restrict our sample to the population at working age (but older than 24). For analyses of the mobility of other groups than the population of working age, we refer to a wide range of existing literature (e.g. Andrew (2004), Angelini and Laferrère (2010)).

Before going through the different covariates that are included in the model, we first discuss the dependent variable, geographical mobility. More specifically, the dependent variable equals 1 if the household moved within the year after period t, conditional on the explanatory variables in period t. In an international perspective, residential mobility is rather low in Belgium. Caldera Sánchez and Andrews (2011b) constructed a comparative study of OECD countries. They show that in 2007, almost 12% of Belgian households had moved over the last 2 years. This is comparable to neighbouring countries as Luxembourg, the Netherlands and Germany but much lower than France and the UK. The Nordic and Anglo-Saxon countries experience typically higher mobility, while mobility is typically low in eastern and southern European countries.

Table 1 contributes more detailed information about the observed mobility in both datasets. Slightly more than 5% of all observed household-years show residential mobility. When making a distinction between short and long distance mobility, we see that the vast majority of moves are local. According to the PSBH dataset, only 0.91% of household-years resulted in a move from one district to another. This equals 15.6% of the total number of moves. EU-SILC does not contain information at the district level but it does on the larger scale of provinces. From the total number of observations, 0.47% concern a move between provinces, which corresponds to 8.8% of the total number of moves. These numbers are clearly lower than the statistics based on the British Household Panel Survey (BHPS) that Böheim and Taylor (2002) report. According to their findings, 17.0% of the total number of moves proceeded between regions (comparable to the Belgian provinces) and 33.1% between Local Authority districts (comparable to Belgian districts). In the PSBH, the households that moved were asked about the main reason for moving house (Table 1, n°4). Only 7.05% of households that responded to this question, stated that the main motivation for mobility was work-related.

Table 1: Some descriptive statistics of mobility

	Move (%)	**Move (Abs.)**	**No move (%)**	**No move (Abs.)**	**Total (Abs.)**
1. Overall fraction of residential mobility					
- PSBH	5.86%	1,039	94.14%	16,703	17,742
- EU-SILC	5.08%	683	94.92%	12,751	13,434
2. Overall fraction of long distance mobility					
- PSBH (between districts)	0.91%	162	99.09%	17,580	17,742
- EU-SILC (between provinces)	0.47%	60	99.53%	13,371	13,434
3. Overall fraction of short distance mobility					
- PSBH (within districts)	4.94%	877	95.06%	16,865	17,742
- EU-SILC (within provinces)	4.64%	623	95.36%	12,811	13,434
4. From the households that moved, which fraction said the main reason for mobility was (PSBH):					
A. Work	7.05%	59	-	-	-
B. The housing itself	69.41%	581	-	-	-
C. Personal reasons	23.54%	197	-	-	-

Source: own calculations; Panel Study on Belgian Households (1992-2002), Universiteit Antwerpen, Université de Liège and EU-SILC (2004-2009), FOD Economie, K.M.O., Middenstand en Energie

To summarize, the descriptive statistics in Table 1 show that only a small fraction of moves is non-local and that the observed mobility is only for a small part motivated by work related circumstances. Four our empirical analysis, the question can be asked which definition of mobility to use. For instance, some studies confined the dependent variable to long distance mobility (e.g. Hughes and McCormick (1985) and Böheim and Taylor (2002)) or mobility for job reasons only (Gardner et al. (2001)). Unfortunately, our two datasets do not allow using a more restricted definition for mobility. First of all, the number of non-local moves is not only small in relative numbers but also in absolute numbers. Only 162 non-local moves are observed in PSBH and 60 in the EU-SILC data. In order to properly estimate an extensive model, the sample size should be much larger. Futhermore, incorporating the reason of mobility would also introduce an important deficiency. It can be deduced from Table 1 that only 4 out of 5 movers responded to this question. The main reason is that in case of attrition in t+1, mobility can be observed for the most part but the reason remains unknown. The attrition can possibly bias the results. Although these limitations might seem unfortunate, we argue that both suggested refinements of the mobility definition are not particularly desirable. Our main research question is more general. We want to investigate the impact of housing tenure on residential mobility. The reason for this mobility or the distance is less important.

The selection of explanatory variables in the model is based on a numerous amount of earlier studies[7]. The applied regressors appear repeatedly in the literature, but sometimes in altered form. We discuss them concisely one by one. Table 2 for PSBH and Table 3 for EU-SILC show the composition for categorical variables and the mean and standard deviation for continuous variables. A distinction is made between the group of movers, non-movers and the total sample in order to provide some first insights into the determinants of mobility. We divide the explanatory variables into 3 broad categories: housing tenure, household characteristics and area characteristics.

As we clarified in the introduction, in this paper *tenure choice* is subdivided into 4 subcategories. We make the distinction between outright owners, owners with a mortgage, tenants paying rent at market rate and tenants paying rent at a reduced rate. In the total population the group of tenants paying rent at market rate is a little over 20%. When only

[7] E.g. Bartel (1979), Hughes and McCormick (1981, 1987), Gardner et al. (2001), Böheim and Taylor (2002), Helderman et al. (2004), Taylor (2007), Tatsiramos (2008), Diaz-Serrano and Stoyanova (2010), Rabe and Taylor (2010a) and Caldera Sánchez and Andrews (2011b),

taking the movers into account, the private tenants count for two thirds of the households-years.

Table 2: PSBH Descriptive Statistics of Explanatory Variables

	No move		Move		Overall	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Tenure choice						
Owner with mortgage	0.4752		0.1627		0.4569	
Outright owner	0.2746		0.0674		0.2624	
Private tenant	0.1876		0.6833		0.2167	
Reduced rent	0.0626		0.0866		0.0640	
Household characteristics						
Age	43.25	10.91	36.96	10.15	42.88	10.97
Family structure						
Single person	0.1881		0.2878		0.1939	
Single parent	0.0589		0.1261		0.0625	
Childless couple	0.4200		0.3012		0.4134	
Couple with children	0.3330		0.2849		0.3302	
Education						
Tertiary	0.4399		0.4398		0.4399	
Secondary	0.3129		0.3147		0.3130	
Less than secondary	0.2471		0.2454		0.2470	
Other						
Foreign nationality	0.1203		0.1578		0.1225	
Monthly income	2,109	1,090	1,866	997	2,094	1,086
Rooms per household member	1.90	1.28	1.83	1.13	1.89	1.28
Years since installation	10.97	7.23	5.88	5.03	10.67	7.22
Area characteristics						
Prov. unemployment rate	8.90	4.33	9.76	4.41	8.95	4.34
Housing trans./cap (t-1)	0.0070	0.0017	0.0069	0.0019	0.0070	0.0017
GVA Construction/cap (t-1)	0.997	0.201	0.985	0.188	0.996	0.200
Population density	971	1738	1354	2135	994	1766
Regional dummies						
Brussels	0.1094		0.1790		0.1135	
Flanders	0.4982		0.4042		0.4927	
Wallonia	0.3924		0.4167		0.3938	

Source: Own calculations; Panel Study on Belgian Households (1992-2002), Universiteit Antwerpen, Université de Liège and EU-SILC (2004-2009), FOD Economie, K.M.O., Middenstand en Energie

Table 3: EU-SILC Descriptive Statistics of Explanatory Variables

	No move		Move		Overall	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Tenure choice						
Mortgagee	0.4191		0.1483		0.4053	
Outright owner	0.3059		0.1057		0.2957	
Private tenant	0.2033		0.6622		0.2266	
Reduced rent	0.0717		0.0837		0.0723	
Household characteristics						
Age	46.73	10.86	39.70	10.96	46.37	10.97
Family structure						
Single person	0.1959		0.3098		0.2017	
Single parent	0.1045		0.1292		0.1057	
Childless couple	0.4428		0.3422		0.4377	
Couple with children	0.2568		0.2188		0.2549	
Education						
Tertiary	0.4579		0.4640		0.4582	
Secondary	0.3671		0.3363		0.3656	
Less than secondary	0.1750		0.1997		0.1762	
Other						
Foreign nationality	0.1615		0.2775		0.1674	
Monthly income	34,304	65,830	28,271	27,681	33,998	64,452
Rooms per household member	2.33	1.36	2.14	1.32	2.32	1.36
Years since installation	14.47	11.31	6.75	7.31	14.08	11.27
Area characteristics						
Prov. unemployment rate	12.25	6.29	13.18	6.57	12.30	6.31
Housing trans./cap (t-1)	0.0065	0.0017	0.0061	0.0019	0.0065	0.0017
GVA Construction/cap (t-1)	1.128	0.277	1.115	0.259	1.128	0.276
Population density	1133	1945	1733	2457	1164	1978
Regional dummies						
Brussels	0.1274		0.2305		0.1327	
Flanders	0.5470		0.4860		0.5439	
Wallonia	0.3256		0.2834		0.3234	

Source: Own calculations; Panel Study on Belgian Households (1992-2002), Universiteit Antwerpen, Université de Liège and EU-SILC (2004-2009), FOD Economie, K.M.O., Middenstand en Energie

Second, we incorporate a wide range of variables that capture *household characteristics*. In order to meet the strict exogeneity assumption, these variables are fixed at the value of the household's first observation in the sample as has been more elaborately discussed in the previous section. We introduce age and its square form to take life cycle effects into account. We also include its square because the relationship is not expected to be linear. We expect a decrease of mobility with age because of declining present discounted wage benefits of mobility while the cost of moving house does not decline (Schwartz, 1973 and Sjaastad, 1962). Because we need one observation per household, we use the age of the oldest member who is not yet 65. Next, a categorical variable is introduced that relates to family structure. The expected effect from having children is ambiguous. We would expect that it becomes more costly and complex to move with a larger household. On the other hand, households might need to move to a larger home to satisfy the household's needs. To take this last consideration into account, we follow the example of Helderman et al. (2004) and Böheim and Taylor (2002) and introduce a proxy for the so-called room-stress. We include the ratio of the number of rooms to the number of household members. If a household enjoys more space, it is expected to be less likely to move. By incorporating this variable, the expected estimate of having children on mobility is unambiguously negative. Cohabiting is expected to have a negative effect on mobility. Following the arguments of Helderman et al. (2004), we combine the properties of cohabiting and whether the household contains children. It is conceivable that having children will have a different impact on singles than on couples. Next, the level of education is included. To incorporate this we take into account the highest acquired degree of one of the household members. We make a distinction between tertiary education, higher secondary education and not having fulfilled secondary education. Last, we control for income and nationality. As to the latter, a dummy equals 1 if at least one household member has a foreign nationality. In the literature, the estimates for income are rather inconclusive, while being foreign is generally found to have a positive effect on mobility.

Finally, the third category contains *area characteristics*, based on aggregate data derived from Cambridge Econometrics data and data from the 'FOD Economie', Belgian Federal Government. We include the provincial unemployment rate in year t as an additional determinant. The expectation is that households have a higher propensity to move when the labour market is depressed. Next, we add a number of proxies to account for housing market conditions: the provincial per capita number of housing market transactions in the year t-1, provincial per capita real gross value added (GVA) of the construction sector in t-1 and last, the population density in the province. These proxies should capture housing supply as a determinant for mobility. In a more liquid housing market, mobility is expected to be more accessible. Last, to control for spatial disparities, we add dummies for the different regions. These capture a range of legislative, cultural and demographic circumstances that could influence the mobility of a household.

4. Results

Table 4 and Table 5 show the results for PSBH and EU-SILC respectively. In contrast to linear models, the estimated coefficients cannot be interpreted in a straightforward way. The size of the partial effects is subject to the selected values of the other regressors. Besides, when we use the panel dimension of the data, the partial effect also depends on the value of the unobserved heterogeneity, μ_i. We follow the suggestion in Wooldridge (2005) and calculate Average Partial Effects (APE's)[8]. This results in one single interpretable estimate for each determinant. For discrete variables, the partial effect equals the difference in probability when the dummy changes from 0 to 1. Accordingly, in case of categorical variables, the APE reveals the difference in moving probability compared to the reference category.

The columns in each table represent the alternative estimation methods as discussed in the previous section: (1) is the Pooled estimator; (2) is the Random Effects estimator; (3) is the so-called Mundlak-approach in which the means of the time-varying regressors are included and (4) is the estimation in which the time since last mobility is added as a supplementary control variable. From a first glance, we can see that the results are quite similar, irrespective of the method. However, the size of the APE's of interest changes rather considerably between the columns. We now discuss the suitability of the different models. At the bottom of the table, Rho indicates the variation that is captured by the unobserved household specific term. Using the RE estimator, this fraction is very low in the PSBH dataset, representing only 0.48% of variation. It is not significantly different from zero. Diaz-Serrano and Stoyanova (2010) obtain a similar result. For PSBH, we can confirm their conclusion that household specific effects are irrelevant and hence the pooled probit model is a more suitable framework compared to the Random Effects estimator. Table 5 shows that Rho equals 11.51% in the EU-SILC case and is significantly different from zero at the 5% level. To compare, Böheim and Taylor (2002) obtained 10% and Rabe and Taylor (2010a) 22%. In contrast to Table 4, unobserved heterogeneity should be accounted for.

[8] The calculation of APE's is very staightforward in the version of Stata®12.1 using the -margins- command.

Table 4: Probit coefficients and selected average partial effects based on PSBH data

	(1) Pooled	(2) RE	(3) Mundlak	(4) Wooldridge
Selected average partial effects				
Tenure choice				
Owner with mortgage	-0.005	-0.005	-0.053(***)	-0.063(***)
	(0.003)	(0.003)	(0.013)	(0.013)
Private tenant	0.131(***)	0.131(***)	0.133(***)	0.118(***)
	(0.007)	*(0.007)*	*(0.014)*	*(0.014)*
Reduced rent	0.048(***)	0.048(***)	0.069(***)	0.054(***)
	(0.008)	(0.009)	(0.019)	(0.018)
Area characteristics				
Prov. unemployment rate	-0.001	-0.001	-0.002	-0.004
	(0.001)	(0.001)	(0.003)	(0.003)
Housing trans./cap (t-1)	1.435	1.434	2.668	5.038
	(2.235)	(2.235)	(7.131)	(7.128)
GVA Construction/cap (t-1)	0.021	0.021	-0.001	0.019
	(0.015)	(0.015)	(0.048)	(0.049)
Population density	0.00007(***)	0.00007(***)	0.00009(***)	0.00009(***)
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Brussels	-0.135(***)	-0.135(***)	-0.382(***)	-0.336(***)
	(0.009)	*(0.009)*	*(0.118)*	*(0.119)*
Wallonia	0.032(***)	0.032(***)	0.037(***)	0.035(***)
	(0.008)	(0.008)	(0.011)	(0.011)
Probit coefficients				
Age	-0.050(***)	-0.050(***)	-0.055(***)	-0.036(**)
	(0.014)	(0.013)	(0.014)	(0.015)
Age squared	0.0004(**)	0.0004(**)	0.0005(***)	0.0003(*)
	(0.0002)	*(0.0002)*	*(0.0002)*	*(0.0002)*
FAM: Single person	0.124(**)	0.124(**)	0.194(***)	0.164(***)
	(0.059)	(0.059)	(0.060)	(0.062)
FAM: Single parent	0.201(***)	0.202(***)	0.246(***)	0.198(***)
	(0.064)	*(0.065)*	*(0.065)*	*(0.067)*
FAM: Couple with children	0.027	0.027	0.051	0.032
	(0.047)	(0.047)	(0.048)	(0.050)
EDU: Less than secondary	-0.002	-0.002	0.098	0.092
	(0.046)	(0.047)	(0.106)	(0.109)
EDU: Tertiary	-0.056	-0.056	-0.077	-0.101
	(0.041)	*(0.041)*	*(0.129)*	*(0.133)*
Foreign nationality	0.052	0.052	0.064	0.054
	(0.049)	(0.049)	(0.050)	(0.051)
Ln(Income)	0.057	0.057	0.011	0.0001
	(0.044)	*(0.044)*	*(0.045)*	*(0.047)*
Rooms per household member	-0.022	-0.021	-0.027	-0.030(*)
	(0.016)	(0.017)	(0.017)	(0.018)
Years since installation (constant)	n/a	n/a	n/a	0.026(**)
				(0.013)
Years since installation (continuous)	n/a	n/a	n/a	-0.049(***)
				(0.012)
Constant	-1.625(***)	-1.631(***)	-1.360(***)	-1.559(***)
	(0.476)	(0.479)	(0.486)	(0.503)
Time dummies	yes	yes	yes	yes
Means of time-varying covariates	no	no	yes	yes
Rho	n/a	0.0048	0.000002	0.000001
Log likelihood	-3,402.82	-3,402.80	-3,330.33	-3,149.63
Number of observations	18,262	18,262	18,262	17,728

Source: own calculations; Panel Study on Belgian Households (1992-2002), Universiteit Antwerpen, Université de Liège.
Note: * (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors. The reference category represents: outright owner, employee, age 35-54, childless couple, higher secondary education, no foreign nationality, in the Region Flanders.

Table 5: Probit coefficients and selected average partial effects based on EU-SILC data

	(1) Pooled	(2) RE	(3) Mundlak	(4) Wooldridge
Selected average partial effects				
Tenure choice				
Owner with mortgage	-0.010(***)	-0.009(***)	-0.044**)	-0.052(***)
	(0.004)	(0.003)	(0.019)	(0.019)
Private tenant	0.096(***)	0.090(***)	0.181(***)	0.181(***)
	(0.008)	*(0.008)*	*(0.025)*	*(0.024)*
Reduced rent	0.031(***)	0.028(***)	0.162(***)	0.161(***)
	(0.009)	(0.008)	(0.028)	(0.027)
Area characteristics				
Prov. unemployment rate	-0.0006	-0.0006	0.0003	-0.0029
	(0.0006)	(0.0006)	(0.0040)	(0.0044)
Housing trans./cap (t-1)	-0.427	-0.300	6.280	7.383
	(1.786)	(1.673)	(4.864)	(5.173)
GVA Construction/cap (t-1)	-0.006	-0.006	-0.016	0.101
	(0.010)	(0.009)	(0.111)	(0.128)
Population density	0.000001	0.000001	0.00005(***)	0.00005(***)
	(0.000002)	(0.000001)	(0.00001)	(0.00001)
Brussels	-0.003	-0.003	-0.003	-0.003
	(0.005)	*(0.005)*	*(0.005)*	*(0.006)*
Wallonia	-0.001	-0.001	-0.001	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Probit coefficients				
Age	-0.045(***)	-0.049(***)	-0.048(***)	-0.044(***)
	(0.015)	(0.017)	(0.016)	(0.016)
Age squared	0.0003(*)	0.0003(*)	0.0003(*)	0.0003(*)
	(0.0002)	*(0.0002)*	*(0.0002)*	*(0.0002)*
FAM: Single person	0.057	0.056	0.068	0.053
	(0.060)	(0.065)	(0.063)	(0.061)
FAM: Single parent	-0.060	-0.069	-0.047	-0.061
	(0.073)	*(0.079)*	*(0.077)*	*(0.073)*
FAM: Couple with children	0.013	0.012	0.009	0.001
	(0.053)	(0.058)	(0.056)	(0.054)
EDU: Less than secondary	0.077	0.081	-0.016	-0.017
	(0.058)	(0.063)	(0.142)	(0.140)
EDU: Tertiary	0.113(**)	0.121(**)	0.108	0.107
	(0.047)	*(0.051)*	*(0.161)*	*(0.159)*
Foreign nationality	0.143(***)	0.148(***)	0.156(***)	0.124(**)
	(0.052)	(0.056)	(0.055)	(0.053)
Ln(Income)	-0.020	-0.020	-0.036	-0.027
	(0.038)	*(0.041)*	*(0.040)*	*(0.039)*
Rooms per household member	-0.016	-0.016	-0.021	-0.020
	(0.018)	(0.019)	(0.019)	(0.018)
Years since installation (constant)	n/a	n/a	n/a	0.083(**)
				(0.042)
Years since installation (continuous)	n/a	n/a	n/a	-0.099(**)
				(0.042)
Constant	-0.057	-0.054	0.135	0.205
	(0.514)	(0.556)	(0.544)	(0.523)
Time dummies	yes	yes	yes	yes
Means of time-varying covariates	no	no	yes	yes
Rho	n/a	0.1151(**)	0.0693(**)	0.00001
Log likelihood	-2,275.65	-2,273.42	-2,237.48	-2,221.00
Number of observations	13,434	13,434	13,434	13,431

Source: own calculations; EU-SILC (2004-2009), FOD Economie, K.M.O., Middenstand en Energie.

Note: * (**) (***) indicates statistical significance at 10% (5%) (1%). Between brackets are estimated standard errors. The reference category represents: outright owner, employee, age 35-54, childless couple, higher secondary education, no foreign nationality, in the Region Flanders.

Both tables show that the main dissimilarities appear when the so-called Mundlak terms are added to the Random Effects Probit model. As argued in the previous section, the results shown in column 2 are inconsistent if the unobserved heterogeneity term is correlated with the dependent variables. Because the extension of the Mundlak approach alters the estimates considerably and the Mundlak terms are jointly significant (not shown in the table), we can confirm our suspicions about the occurrence of unobserved heterogeneity as described in Section 2. The fourth column shows that conditioning the model on the duration spent in the residence, alters the magnitude of the estimated APE's rather strongly. The highly significant coefficient of "years since installation (constant)" proves that controlling for time spent in the residence, helps explaining the model. Its continuous counterpart reveals a significant negative coefficient. The longer a household remains in the same house, the more restrained it is for future mobility. This corresponds with the expectations as discussed in Section 2. We consider this last specification to be the most suitable model.

The results are very much in line with the expectations and similar in both datasets. The upper half of the tables shows the APE's of the key explanatory variables. Consistent with the earlier research that we described in the introduction, the results show that private tenants are the most likely to move, followed by tenants paying reduced rent. Unlike the results of Caldera Sánchez and Andrews (2011b) for Belgium, we do find a significant difference between outright owners and mortgagees. Outright homeowners appear to have a higher propensity to move (of 6.3% for PSBH, 5.2% for EU-SILC) which is in line with the estimates of Böheim and Taylor (2002) and Rabe and Taylor (2010a). The area characteristics have only limited explanatory power. The results do not provide evidence that households are more mobile if the aggregate unemployment rate is high, but neither did the aforementioned literature. From the proxies that we introduced to capture housing market conditions, only population density renders a significant coefficient. Households living in more densely populated areas experience higher mobility. A possible reason is the higher liquidity of the housing market. Finally, the regional dummies have significant estimates, but only in the PSBH dataset. The reason for the divergent outcome of both datasets is impossible to deduce. One would rather expect the opposite because Belgian housing policy was mainly decentralized in 2002. Of course, these regional dummies capture much more than this, so it is uncertain to what extent these APE's demonstrate policy effects.

The lower halves of Table 4 and 5 show the probit coefficients of the control variables. As clarified before, the estimates of these variables cannot be interpreted structurally because the values are fixed to the first observation of each household. Therefore, showing the APE's is otiose. For both datasets, the age categories have high explanatory power. Next we observe that the degree of statistical significance of the household characteristics differs substantially between both datasets. Whereas for the PSBH data family structure and room stress help to determine mobility, nationality is the most effective control in case of the EU-SILC data. Income seems to have no influence in both cases, which is consistent with Kan (2007) and Diaz-Serrano and Stoyanova (2010).

As announced in Section 2, we explicitly test whether or not the strict exogeneity assumption holds for the housing tenure dummies. When added to the Wooldridge regressions, we observe that the leads of some of these dummies do show significant coefficients. This result suggests that we fail to meet the assumption and caution is required when interpreting the estimated APE's. These are possibly driven by the correlation between 'shocks' to mobility and contemporaneous or future values of the housing tenure choice.

5. <u>Conclusions</u>

In this paper, we analyse the determinants of residential mobility in a large panel of Belgian households in 1994-2009. Like most papers in this literature, we find that - ceteris paribus - tenants are more mobile than owners. Neither of the two groups are homogeneous, however. Homeowners with a mortgage are less mobile than outright owners. Among tenants, those paying a reduced rate are significantly less mobile than tenants on the private market. The magnitude of the estimated average partial effects reveals that housing tenure is an economically significant determinant of mobility. Comparing the most and the least mobile groups (private tenants and mortgagees), we observe a difference of 18 to 23%-points in the probability per year to move. The hampered mobility of homeowners (especially with a mortgage) may have a large unfavourable effect in a country with a severely high homeownership rate.

Our estimation methods build on the Mundlak approach as applied in the previous literature (e.g. Tatsiramos (2009), Rabe and Taylor (2010a) and Diaz-Serrano and Stoyanova (2010)). Using the Wooldridge approach, we extend this estimation method to control for state-dependence. Both model specifications require strict exogeneity of the explanatory variables. Although the previous studies do not acknowledge this condition to obtain unbiased results, we have tried to avoid endogeneity by fixing as many potentially endogenous variables as possible at their initial value in the first period of the observation. Since this procedure implies, however, that a structural interpretation of the estimated coefficients is no longer possible, we could not impose it on the housing tenure variables. Tests reveal that in the end the strict exogeneity assumption may still be violated, implying that we cannot exclude the possibility of some bias in our estimated coefficients. The resulting bias probably manifests itself in all previous papers that analyse the effect of housing on mobility. A solution to this problem is not straightforward but progress may recently have been made by Biewen (2009). He developed a dynamic model (analysing poverty status) that explicitly allows for feedback from the dependent variable to future values of the explanatory variables. This might be a promising starting point for further research in order to avoid the remaining bias.

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CHAPTER 4

Does homeownership lead to longer unemployment spells? The role of mortgage payments

Does homeownership lead to longer unemployment spells?
The role of mortgage payments

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Abstract

This paper examines the impact of housing tenure choice on unemployment duration in Belgium using EU-SILC micro data. We contribute to the literature in distinguishing homeowners with mortgage payments and outright homeowners. We simultaneously estimate unemployment duration by a mixed proportional hazard model, and the probability of being an outright homeowner, a homeowner with mortgage payments or a tenant by a mixed multinomial logit model. To be able to correctly identify the causal influence of different types of housing tenure on unemployment duration, we use instrumental variables. Our results show that homeowners with a mortgage exit unemployment first. Outright owners stay unemployed the longest. Tenants take an intermediate position. Moreover, our results reveal the different share of mortgage holders within the group of homeowners as a possible explanation for the discrepancy between former contributions to this literature.

JEL classification: C41, J64, R2.
Keywords: unemployment, housing tenure, duration analysis.

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1. Introduction

Does homeownership impair an individual's labour market outcome? Seminal work by A.J. Oswald (1996, 1997) suggests that it does. A key element in his view is that high costs of buying and selling homes make homeowners less geographically mobile than tenants. As a result, in case of job loss, the number of suitable vacancies within homeowners' reach will be much smaller. Their exit rate from unemployment will therefore be lower. Empirically, many studies confirm Oswald's claim that homeowners are geographically less mobile than tenants (see, e.g., Hughes and McCormick, 1981, 1987; Böheim and Taylor, 2002; Caldera Sánchez and Andrews, 2011; Isebaert, 2013). Nevertheless, direct research into the relationship between housing tenure choice and labour market outcomes using micro data does generally not find that homeowners have worse labour market perspectives than tenants. Battu et al. (2008) for example find no significant difference in the speed of transition from unemployment into employment among homeowners versus private tenants in the UK. Munch et al. (2006) even observe a faster exit from unemployment into employment among owners than among tenants in a large panel of Danish individuals, while van Leuvensteijn and Koning (2004) find a significant negative impact of homeownership on the risk of becoming unemployed in the Netherlands. These three papers are important not only for their results, but also methodologically. Each of them adequately deals with the impact of individuals' unobserved characteristics which may affect both their labour market situation and their tenure choice.

From a theoretical point of view, various explanations have been advanced in these and other micro studies to rationalize the better perspectives of owners on the labour market. Coulson and Fisher (2002) emphasize the importance of social networks in the search for work. Homeowners tend to invest more in their social network which improves their local job opportunities. Munch et al. (2006) add that because of high moving costs, homeowners have a lower reservation wage and a higher search intensity for local jobs. According to van Leuvensteijn and Koning (2004) and Munch et al. (2008) homeowners are willing to invest more in their job, in order to maximize the probability of staying in the local job. Accordingly, firms anticipate longer employment duration of homeowners and so are willing to invest in firm-specific training. This further increases firm-specific productivity of the homeowner.

This paper investigates the impact of housing tenure choice on unemployment duration in Belgium, using EU-SILC micro data. Our basic research question is therefore the same as that of Munch et al. (2006) and Battu et al. (2008). We also follow these studies in their choice of methodology. Our main contribution to the literature is that we distinguish different types of homeowners. Whereas Munch et al. (2006) only make the broad subdivision between homeowners and non-homeowners, and Battu et al. (2008) split up the second group into public and private tenants, we distinguish homeowners with mortgage payments and outright owners. For Belgium, where the rate of homeownership is close to 70%, this is clearly the most relevant distinction. About two thirds of all homeowners are mortgagees, about one third are outright owners[1]. From the point of view of the Oswald hypothesis, different labour market outcomes between both groups of owners should not be expected. The search and transaction costs that are associated with moving are similar for outright owners and mortgagees[2]. The motivation for not treating homeowners as a homogeneous group lies elsewhere. Rouwendal and Nijkamp (2010) embed the distinction between both types of owner-occupiers in a theoretical framework explaining search behaviour. Building on Munch et al. (2006), they develop a model with both local and non-local labour markets. Moving costs both decrease owners' nonlocal job search (the Oswald effect) and increase their local search. The net effect of moving costs in Rouwendal and Nijkamp is that owners on average experience longer unemployment duration. They further advance this theoretical model by introducing housing costs. The fraction of the wage that is not spent on housing goes to nondurable consumption which determines utility. Decreasing marginal utility explains why the unemployed will have a higher search intensity when housing costs are high. This result may critically affect the earlier theoretical outcome. According to Rouwendal and Nijkamp's model, if housing costs are lower for homeowners than for tenants (as is the case for outright homeowners), owners will experience even longer

[1] Private rental and social housing account for about 23% and 7% of housing supply respectively. In the UK that is 15.6% and 18% (Pittini and Laino, 2011).

[2] Unsurprisingly, the above mentioned empirical literature studying geographical mobility leaves us with mostly ambiguous answers to the question whether outright owners or mortgagees are more geographically mobile. For example, in a cross-section of 23 OECD countries, Caldera Sánchez and Andrews (2011) find outright owners to be less residentially mobile than owners with mortgage payments in 15 countries. They observe the opposite in 4 other countries. In 4 last countries, one of which is Belgium, there is no significant difference between outright owners and mortgagees. Isebaert (2013) by contrast uses panel data and finds mortgagees to be less geographically mobile than outright owners in Belgium. The only robust empirical result across studies seems to be that tenants are more residentially mobile than owners.

unemployment duration. The Oswald effect is then reinforced by a (low) housing cost effect. If housing costs for owners are higher than for tenants (as may be the case for mortgagees), the reverse occurs. The unemployed owners' search intensity will then rise, and their unemployment duration falls. The Oswald effect may then be beaten by a (high) housing cost effect. Building on this theory, one may therefore expect the fastest exit from unemployment for mortgagees, and the slowest for outright owners. Tenants may take an intermediate position[3].

Empirically, to the best of our knowledge, only Goss and Phillips (1997) and Flatau et al. (2003) made the distinction between outright owners and owners with a mortgage to address differences in unemployment duration before. Both papers find higher exit rates from unemployment for homeowners with a mortgage. Methodologically, however, the empirical models used in these studies do not adequately handle the potential endogeneity bias that may arise if a person's unobserved characteristics affect both his unemployment duration and housing tenure. Munch et al. (2006) provide the example of a person who is inherently less mobile because of preference for stability. On the one hand, this person will be inclined to buy a house and settle in a chosen area. On the other hand, the stability-preferring individual is less willing to move for job reasons, extending the duration of an unemployment spell. One might falsely interpret the combination of these events as a causal relationship from homeownership to longer unemployment. To resolve this issue, we adopt an econometric framework that builds on those used by van Leuvensteijn and Koning (2004), Munch et al. (2006) and Battu et al. (2008). More precisely, we simultaneously estimate unemployment duration by a mixed proportional hazard model, and the probability of being an outright homeowner, a homeowner with mortgage payments or a tenant by a mixed multinomial logit model. To be able to correctly identify the causal influence of different types of housing tenure on unemployment duration, we use instrumental variables (exclusion restrictions). These are variables that influence housing tenure but do not directly

[3] Available data for Belgium support the idea that housing costs differ significantly by tenure situation. Vastmans and Buyst (2011) reveal that monthly mortgage payments account for 24.6% of a household's net monthly income, on average. Housing costs of outright owners by contrast are limited to the maintenance costs. As to the distinction between homeowners with a mortgage and tenants, Heylen et al. (2007) report a mean rental price in the Flemish region in 2005 of 396€, while the mean mortgage payment was equal to 564€. The latter clearly represents the heaviest burden on the household budget. Furthermore, tenants experience lower costs of maintenance since the depreciation of a dwelling is to a great extent at the expense of the owner.

affect unemployment duration. Finding good instruments is often a delicate task in this literature. We contribute by adding a new instrument, which is the relative price of buying to renting a house at the moment in the past that people signed the contract underlying their current tenure.

This paper is the first to analyse the research question at stake for Belgium. For several reasons Belgium may be a very interesting case to test the link between housing and labour market situation at the micro level. The rate of homeownership is considerably higher than in the countries analysed in the aforementioned studies (van Ewijk and van Leuvensteijn, 2009). Furthermore, Belgian tax rates on housing transactions are among the highest in the world (European Mortgage Federation, 2010). Also Belgian labour market characteristics differ strongly from those in the previously investigated countries. To mention one, unemployment benefit duration is much longer (OECD, 2013). Taking into account all these considerations, if there were one country to expect a strong Oswald effect, it would be Belgium. Recent macroeconomic work also confirms this. Using aggregate data of Belgian districts since 1970, Isebaert et al. (2013) find strong empirical evidence in favour of the Oswald hypothesis.

In accordance with the aforementioned theoretical expectations in the spirit of Rouwendal and Nijkamp (2010), our empirical results prove that homeowners are not a homogeneous group. The result found by Munch et al. (2006) that homeowners have shorter unemployment spells than tenants, only applies to homeowners with a mortgage. Outright owners by contrast remain unemployed the longest. Not having to pay rent or to repay a mortgage seemingly decreases the search intensity of an individual. This result survives various robustness checks. For example, it does not depend on the specific exclusion restrictions that we impose on the model. Neither is it conditional on the age of the individuals in our sample: it also holds if we restrict the sample to owners younger than 50.

Our results may transcend the single Belgian case. A possible explanation for the discrepancy between the results of Munch et al. (2006) for Denmark and Battu et al. (2008) for the UK is the different share of mortgage holders within the group of homeowners. In Denmark the fraction of mortgagees is about 73%. Therefore, it is not surprising that the positive effect for this subgroup dominates the negative effect for outright owners, when no distinction is made between both groups. In the UK the fraction of mortgagees in the group of owners is (only) about 56%. Positive and negative effects on the exit rate from unemployment from both subgroups may then cancel out.

The structure of this paper is as follows. In the next section we provide the reader with an introduction to the dataset and some descriptive analyses. The specification of our methodological framework is included in Section 3. In Section 4 we show the results of our estimations. A final section concludes.

2. Data and descriptive statistics

To analyse unemployment spells in Belgium, we use the recent dataset of the European Union Statistics on Income and Living Conditions (EU-SILC). This survey provides longitudinal data of topics such as labour market conditions, education, housing tenure, income and social exclusion. It was designed in order to replace the less harmonized European Community Household Panel (ECHP). By using the EU-SILC data, we are able to analyse household behaviour in the period 2003-2008. A prominent characteristic of this survey is the rotating sample design. The first quarter of the sample is replaced each year. Hence, the sample of households is fully renewed after four years.

We use the spells of unemployment that start after a period of employment (i.e. left-censored spells are withheld). A spell can end with re-employment or with right-censoring. The latter can be the result of an activity status different from (un)employment[4], or can be due to non-observation in the next period. Consequently, unemployment spells that outreach the period of observation, are automatically right-censored. Only the first unemployment spell of each individual is included. During the 6 year time interval, we observe 1048 unemployment spells of which 26 are dropped from the sample because of missing values for one or more of the explanatory variables. Yet another 9 spells are filtered out for the individuals indicating they enjoy “free housing accommodation”. From the remaining 1013 unemployment spells, 557 spells are fully recorded and 456 spells are right-censored.

[4] Possible destinations are retirement, being permanently disabled or taking up domestic tasks and care responsibilities.

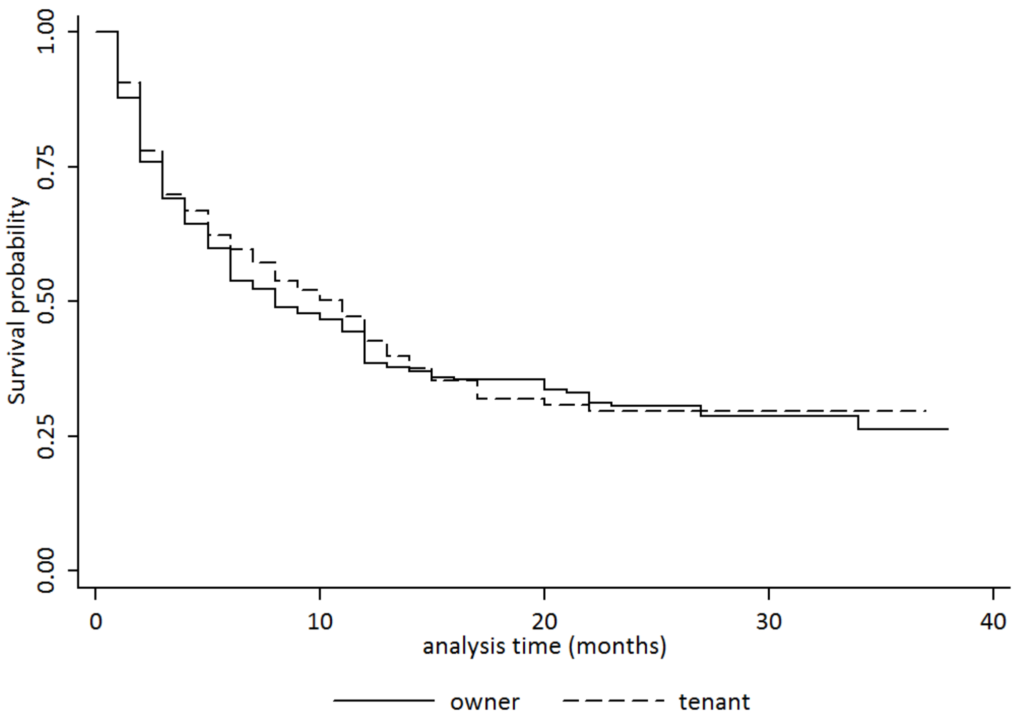
In the EU-SILC dataset, the labour market status is observed monthly. For comparison, it is measured with a weekly frequency in the Danish dataset of Munch et al. (2006). Labour market observations in the BHPS used by Battu et al. (2008) are also monthly. Figure 1 reports non-parametric Kaplan-Meier estimates of the monthly transition out of unemployment by housing status at the start of the unemployment spell in our dataset. Panel A illustrates that owners and tenants show similar transition patterns when we merge outright owners and owners with mortgage payments into one group. By contrast, when we distinguish the latter two categories of owners, as presented in Panel B, we find clear differences between the three housing options. Outright owners have, on average, the longest unemployment spells (with a median duration of 33 months). Tenants and mortgagees have shorter spells with a median duration of 8 respectively 5 months. However, since this comparison does not take selection on neither observable nor unobservable characteristics into account, we cannot conclude from this descriptive evidence that the transition out of unemployment happens slower for outright owners. These particular individuals might have very low chances to leave unemployment fast because of other factors that are dominant within the group of outright owners. The econometric method that we apply in this paper takes the selection on (un)observable characteristics into account and leads therefore to a better founded answer to our research question.

The mean and standard deviation of the explanatory variables used in our analysis are listed in Table 1. As a matter of illustration, these two statistics are shown for each housing status separately as well. For all the explanatory variables in both the unemployment duration model and the housing status model, we use their value at the start of the unemployment spell, and then keep it constant. If these variables were not kept constant, the assumption of strict exogeneity would be violated due to the possibility of reverse causality (see the next section for a more extensive elaboration on this).[5] EU-SILC measures the status of all these explanatory variables with a yearly frequency, at the start of each calendar year (around March). Also the housing status is measured with a yearly frequency. Given our monthly observations of the labour market status, we interpolate in

[5] The only explanatory variable that we allow to vary during unemployment spells is the regional unemployment rate. This variable is strictly exogenous all the way. It contributes to the model by capturing the business cycle at the regional level. Belgium consists of three regions (Flanders, Wallonia and Brussels).

Figure 1: Kaplan-Meier estimates – unemployment duration by housing status

A. Owners versus tenants



B. Mortgagees versus outright owners versus tenants

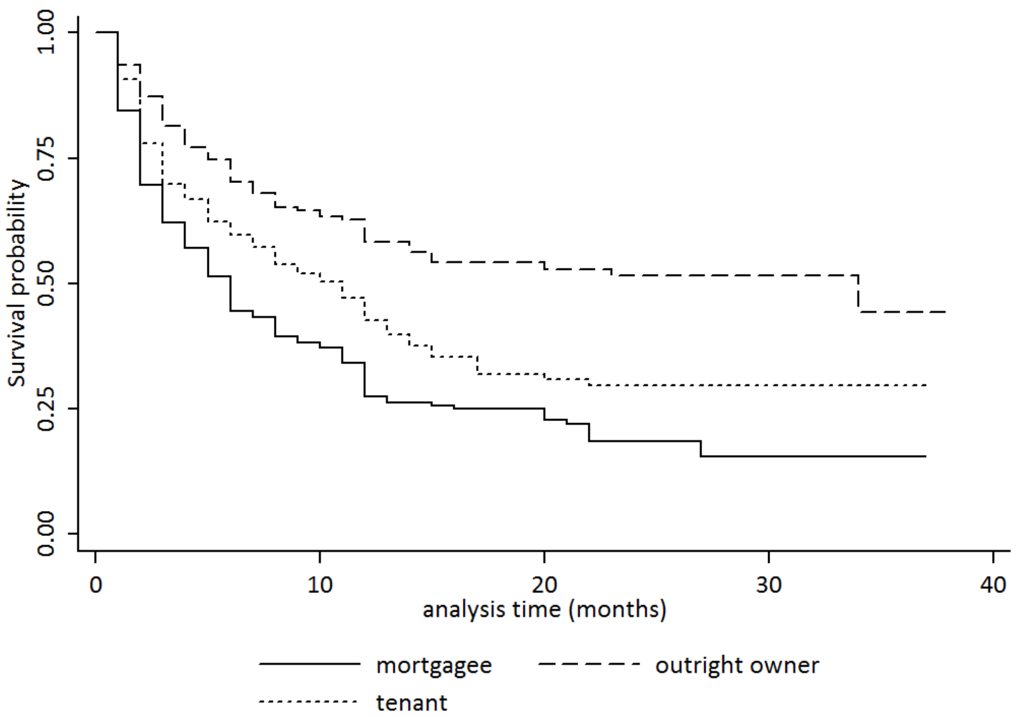


Table 1: Descriptive Statistics of Explanatory Variables								
	Overall		**Tenants**		**Outright owners**		**Mortgagees**	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Housing tenure categories								
Tenant	0.35	(0.48)						
Outright owner	0.23	(0.42)						
Mortgagee	0.42	(0.49)						
Explanatory variables used in both unemployment duration and housing equations								
Woman	0.57	(0.49)	0.59	(0.49)	0.46	(0.50)	0.62	(0.48)
Foreign nationality	0.15	(0.36)	0.23	(0.42)	0.07	(0.25)	0.14	(0.35)
Age 16-24 years	0.09	(0.29)	0.14	(0.35)	0.06	(0.24)	0.07	(0.25)
Age 25-34 years	0.31	(0.46)	0.36	(0.48)	0.15	(0.36)	0.36	(0.48)
Age 35-49 years	0.33	(0.47)	0.33	(0.47)	0.18	(0.38)	0.41	(0.49)
Age ≥ 50 years	0.27	(0.44)	0.16	(0.37)	0.61	(0.49)	0.17	(0.37)
Low educated	0.27	(0.44)	0.32	(0.47)	0.28	(0.45)	0.22	(0.42)
Middle educated	0.40	(0.49)	0.43	(0.50)	0.44	(0.50)	0.37	(0.48)
High educated	0.33	(0.47)	0.26	(0.44)	0.28	(0.45)	0.41	(0.49)
Cohabiting partner	0.66	(0.47)	0.51	(0.50)	0.65	(0.48)	0.80	(0.40)
Working partner	0.42	(0.49)	0.30	(0.46)	0.27	(0.45)	0.61	(0.49)
Having children younger than 18	0.52	(0.50)	0.46	(0.50)	0.24	(0.43)	0.72	(0.45)
Densely populated area	0.54	(0.50)	0.67	(0.47)	0.49	(0.50)	0.45	(0.50)
Brussels	0.12	(0.33)	0.19	(0.40)	0.09	(0.28)	0.08	(0.27)
Flanders	0.53	(0.50)	0.50	(0.50)	0.58	(0.49)	0.53	(0.50)
Wallonia	0.34	(0.48)	0.30	(0.46)	0.33	(0.47)	0.39	(0.49)
Unemployment rate (province)	0.12	(0.06)	0.13	(0.06)	0.12	(0.06)	0.12	(0.06)
Unemployment rate (region)	0.11	(0.06)	0.12	(0.06)	0.11	(0.05)	0.11	(0.05)
Explanatory variables used only in housing equations								
% homeowners (province)	0.67	(0.10)	0.65	(0.12)	0.68	(0.09)	0.68	(0.09)
House price to rent ratio in year of contract (province)	1.47	(0.63)	1.79	(0.78)	1.20	(0.43)	1.36	(0.45)

Source: own calculations based on EU-SILC data, except for unemployment rate (VDAB, FOREM, Belgostat, Vlaamse Arbeidsrekening), homeownership rate (Social-Economic Survey 2011) and house price to rent ratio. (FOD Economie, Belgian Federal Government)
A more detailed definition of each variable is given in appendix A.1.

the spirit of van Leuvensteijn and Koning (2004) and Battu et al. (2008) the yearly observations for the explanatory variables into monthly observations. We assume that the monthly values from October of year *y-1* until September of year *y* equal the observed yearly value in year *y.*[6] This may unavoidably cause some measurement errors. Housing status may in some cases be misperceived. As an example, it might be possible that an individual

[6] The interpolation that we impose assigns the yearly observation in EU-SILC s to six months before and six months after the moment of measurement (around March). This also brings the advantage of a larger sample. When new households enter the panel in year *y*, data is collected also about their labour market situation in the twelve months of *y-1*. Spells that start in October of *y-1* can therefore also be included in our sample.

becomes unemployed in October of year *y-1* and changes tenure in December of that year. In that case our interpolation would imply a wrong value for the housing status variable related to this unemployment spell. Van Leuvensteijn and Koning (2004) also recognize this possibility of measurement error, but state that there are no strong a priori beliefs that these errors lead to an important bias in the estimation results. Considering the results of a sensitivity analysis that we did, we agree. More precisely, we imposed for the yearly observed explanatory variables an alternative interpolation, namely that the monthly values from January until December of year *y* equal the observed yearly value of year *y*. Estimating our model for exactly the same sample (including the maximum number of unemployment spells that can be included under both types of interpolation), our results are very similar. Estimated values for the key coefficients in our model differ by much less than one standard error (details are available upon request).

Table 1 shows that, concerning the housing status, mortgagees constitute the largest fraction in our dataset, followed by tenants. When inspecting the explanatory variables used in both unemployment duration and housing equations by housing status at the start of the unemployment spell, we see that the subsample of outright owners contains relatively more individuals (61%) who are older than 50. As re-employment chances for the elderly are relatively low in Belgium (OECD, 2012), this immediately provides one example of a factor that could have biased the descriptive evidence in Figure 1. When further comparing outright owners and mortgagees, it can be observed that the latter group comprises relatively more female, foreign, high-educated and cohabiting individuals. In addition, compared to both groups of owners, more tenants have a foreign background, are low-educated, are single and are living in a densely populated area.

The lower part of Table 1 shows the two variables that serve as instruments in order to control for the endogeneity of housing tenure. First, we follow van Leuvensteijn and Koning (2004) and Munch et al. (2006, 2008) and introduce the percentage of homeowners in the province into our model. This fraction ought to have a positive effect on the probability of becoming a homeowner. The validity of this instrument is discussed thoroughly by van Leuvensteijn and Koning (2004). Note, however, that Coulson and Fisher

(2009) challenge this validity[7]. We will therefore also conduct a sensitivity analysis without this instrument. We iterate this exercise for the other instrument as well. As our second instrument, we use the ratio of the market price of houses to the rental price at the level of the province, and in the year of signing the rental contract for tenants or the year of purchase for homeowners. When buying a house is relatively inexpensive in comparison to renting, the probability of becoming a homeowner instead of a tenant will increase. Furthermore, this instrument contributes to explaining the probability of being an outright owner versus a mortgage holder. When house prices are relatively high, households will be compelled to borrow larger amounts. One can expect this to imply longer repayment periods, reducing the probability that individuals will be outright owners. Since this price ratio is computed at the aggregate provincial level and concerns the past, the assumption of exogeneity is respected.

3. Methodology

3.1. Model

In order to investigate the effect of housing status on the duration of unemployment, we adopt an econometric framework that builds on those presented by van Leuvensteijn and Koning (2004), Munch et al. (2006) and Battu et al. (2008). On the one hand, the part of the model that describes the transition into employment is specified as a mixed proportional hazard model. On the other hand, given the potential endogeneity of the housing status, for which the former contributions have given evidence, we simultaneously model the probability of being an outright homeowner, a mortgagee or a tenant as captured by a mixed multinomial logit model. We allow that the unobserved heterogeneity captured in both models is mutually correlated.

Our model differs from the models of the aforementioned studies in three main aspects. First, in order to disentangle the effect of being a homeowner with or without mortgage payments, we model the housing status as a multinomial logit model instead of a

[7] Coulson and Fisher emphasize external effects. Regional homeownership rates may affect wage setting and other costs of doing business in a region. This may affect individuals' chances on the labour market. The use of regional homeownership rates as exclusion restriction would then be invalid.

binary logit model. Battu et al. (2008) used the same practice to disentangle the diverging influence of social renting and private market renting. Second, like for all explanatory variables, we model the housing status only at the start of the unemployment spell and use only this (time-constant) status to explain unemployment duration. Our procedure is in contrast with the former contributions which model the housing status for each month of the unemployment spell and include this time-varying housing status variable in the unemployment duration model. We believe, however, that the latter approach may lead to an endogeneity bias as a change in housing status during the unemployment spell might be caused by the unemployment duration. Third, since in our data we do not measure time continuously but on a monthly basis, we take this time-grouping explicitly into account in the specification of the model and - ipso facto - of the likelihood function. The alternative option is to estimate a pure continuous time model on these time-grouped data as if the data were continuous. Although adopted by most of the aforementioned studies, this approximation may lead to substantial estimation biases. Gaure et al. (2007, p.1178) argue, based on their extensive Monte Carlo assessment of the Timing of Events approach, that this is due to the approximation's inherent failure in locating the appropriate unobserved heterogeneity distribution.

3.1.1. Unemployment duration model

In our unemployment duration model, the time interval Δt is normalized to one month. The hazard rate[8] into employment is specified as follows[9]:

$$\vartheta\left(t \middle| \mathbf{x}, z_1, z_2, v\right) = \lambda\left(t\right) \exp\left(\mathbf{x}'\boldsymbol{\beta} + z_1\delta_1 + z_2\delta_2 + v\right), \tag{1}$$

where t is the elapsed duration since the individual became unemployed. $\boldsymbol{x}$ is the vector of observed individual characteristics introduced in the previous section and v is a component capturing unobserved heterogeneity. The baseline hazard $\lambda(t)$, representing the duration dependence in the hazard rate, is specified as a piecewise constant non-parametric function. Last - and most important - the dummy variables z_1 and z_2 capture whether the individual is a tenant respectively a homeowner without mortgage payments at the start of the

[8] The hazard rate is defined as the probability to flow into employment at date t conditional on being unemployed up to t. See Kiefer (1988) for an introduction into duration analysis.

[9] To avoid cumbersome notation, we ignore that the regional unemployment rate is a time-varying covariate.

unemployment spell (being a homeowner with mortgage payments is the reference category). These variables indicate the causal effect of a particular housing status at the start of the unemployment spell on the transition rate out of unemployment afterwards.

3.1.2. Housing status model

The probability of each housing status type at the start of the unemployment spell is specified by a multinomial logit model with unobserved effects:

$$\Pr\left(y = h \middle| \tilde{\mathbf{x}}, u_1, u_2\right) = \frac{\exp\left(\tilde{\mathbf{x}}'\boldsymbol{\alpha}_{\mathbf{h}} + u_h\right)}{1 + \exp\left(\tilde{\mathbf{x}}'\boldsymbol{\alpha}_{\mathbf{1}} + u_1\right) + \exp\left(\tilde{\mathbf{x}}'\boldsymbol{\alpha}_{\mathbf{2}} + u_2\right)} \tag{2}$$

in which $h = \{1,2\}$ and $y = 3 - 2z_1 - z_2$. Furthermore, u_1 and u_2 represent the unobserved heterogeneity in the housing status model. The probability of the reference housing status, i.e. homeowner with mortgage payments, is then given by:

$$1 - \Pr\left(y = 1 \middle| \tilde{\mathbf{x}}, u_1, u_2\right) - \Pr\left(y = 2 \middle| \tilde{\mathbf{x}}, u_1, u_2\right). \tag{3}$$

$\tilde{\boldsymbol{x}}$ is a vector containing $\boldsymbol{x}$ supplemented with the set of additional variables only affecting the housing status on which we elaborated in the previous section. This exclusion restriction is an important issue with respect to the econometric identification of the housing status effect.[10] Therefore, as a sensitivity analysis we will re-estimate the model for subsets of the instruments.

3.2. Estimation

3.2.1. Likelihood conditional on unobserved heterogeneity distribution

The coefficients of the presented model are estimated by maximum likelihood estimation. We assume that all sources of correlation between the unemployment duration and the housing tenure processes - beyond those captured by the observed explanatory variables - can be represented by the (time-invariant and individual-specific) unobserved heterogeneity terms. We first derive the likelihood contributions of these two processes conditional on the unobserved components u_1, u_2 and v.

[10] The alternative identification strategy is to exploit the multiple spell feature of the data which is, however, not an option given that we observe only few unemployment spells during which the individual's housing status mutates.

Concerning the unemployment durations we assume that the censoring times are stochastically independent of the corresponding job durations and the explanatory variables. The conditional likelihood of *T*, which is the unemployment duration as observed in the dataset, of a particular individual can be described as[11]:

$$f_T\left(T\middle|\mathrm{x},z_1,z_2,v\right)=\left[\exp\left[-\sum_{t=1}^{T-1}\vartheta(t)\right]-\exp\left[-\sum_{t=1}^{T}\vartheta(t)\right]\right]^{(1-c)}\exp\left[-\sum_{t=1}^{T}\vartheta(t)\right]^{(c)}. \tag{4}$$

This equation expresses the probability of leaving unemployment between T-1 and T (first factor of the RHS) if T is not censored, i.e. if *c* is 0. If *T* is censored, i.e. if *c* is *1*, the likelihood of *T* equals the survival probability.

The individual likelihood of *y*, the housing status at the start of the unemployment spell of an individual, is given by:

$$\Pr\left(\mathrm{y}\middle|\tilde{\mathbf{x}},u_1,u_2\right)=$$

$$\Pr\left(y=1\middle|\tilde{\mathbf{x}},u_1,u_2\right)^{z_1}\Pr\left(y=2\middle|\tilde{\mathbf{x}},u_1,u_2\right)^{z_2}\left[1-\Pr\left(y=1\middle|\tilde{\mathbf{x}},u_1,u_2\right)-\Pr\left(y=2\middle|\tilde{\mathbf{x}},u_1,u_2\right)\right]^{(1-z_1-z_2)} \tag{5}$$

3.2.2. Integrated likelihood

To obtain the unconditional likelihood contributions, we integrate the conditional contributions over the unobserved heterogeneity distribution. In this respect, we adopt a non-parametric discrete distribution by analogy with Heckman and Singer (1984).[12] We estimate, in the spirit of van den Berg et al. (2002), our model for an optimal number *K* - optimal according to reliable information criteria - of heterogeneity types in the population under investigation. Their proportions are specified as logistic transforms:

$$p_k=\frac{\exp(q_k)}{\sum_{j=1}^{K}\exp(q_j)}$$ *, with k = [1,K] and q_k* parameters to be estimated (q_1 normalized to *0*). (6)

[11] To avoid cumbersome notation, we simplified the notation for theta.

[12] The methodology as advocated by these authors boils down to the assumption that a sample consists of a finite number of subsamples with different levels of time-invariant unobservable effects. Then, for all subsamples the corresponding proportions are estimated as well as the impact of the unobserved differences on the outcomes.

Besides the estimation of these proportions, this approach induces the estimation of one mass point (location) for u_1, u_2 and v for each heterogeneity type: u_{1k}, u_{2k} resp. v_k (u_{11}, u_{21} resp. v_1 are normalized to 0)[13].[14] Hence, the likelihood for an agent i is:

$$l_i = \sum_{k=1}^{K} p_k \cdot f_T\left(T \middle| \mathrm{x}, z_1, z_2, v\right) \cdot \Pr\left(\mathrm{y} \middle| \tilde{\mathbf{x}}, u_1, u_2\right). \tag{7}$$

We can then write the unconditional log-likelihood as the sum of the unconditional individual log-likelihood contributions:

$$L = \sum_{i=1}^{N} l_i . \tag{8}$$

4. Results

4.1. Basic results

Table 2 shows our main estimation results of the model. The Akaike Information Criterion (AIC) indicates an optimal number of two heterogeneity types (K=2)[15]. Homeowners with mortgage payments (who are the reference group) have ceteris paribus the shortest unemployment spells. Outright owners, by contrast, stay unemployed the longest. Their monthly probability to be re-employed is 39% lower than the re-employment probability of owners with mortgage payments[16]. Our results are consistent with the intuition that having to make a monthly payment increases the incentive of finding a job. Tenants have a 21% lower probability to exit from unemployment each month compared to mortgagees.

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[13] We impose this normalisation since we allow for a constant term in the vector of observed characteristics ***x***.

[14] We take both the locations and the probabilities of the mass points to be unknown parameters without constraining the correlation between u_1, u_2, and v. Allowing only perfect correlation or no correlation or a priori limiting the number of heterogeneity types to an arbitrary number – the latter constraint is adopted in most of the mentioned former contributions – may lead to biased estimates, as shown by Gaure et al. (2007). The estimation procedure for gathering the probabilities and locations of the mass points is implemented according to the latter authors.

[15] Table A.2 in the Appendix reveals that the alternative information criteria (Hannan-Quinn Information Criterion and Bayesian Information Criterion) indicate an optimal number of only 1 type (K=1). Following the argument in Gaure et al. (2007), we believe that the AIC is preferable when the sample is relatively small. Nevertheless, we also report in Table A.3 in the Appendix (column 1) the estimation results of the main coefficients in our model when we allow only one single heterogeneity type. The results are very similar to those obtained from estimation with K=2. Note that Battu et al. (2008) also model two heterogeneity types. Van Leuvensteijn and Koning (2004) specify three, Munch et al. (2006) no less than eight.

[16] 1 – exp(-0.50) = 0.39.

Table 2: Unemployment duration and housing model – estimation results									
	Exit to employment			**Tenant**			**Outright owner**		
Explanatory variables									
Tenant	**-0.24**	**	**(0.11)**						
Outright owner	**-0.50**	***	**(0.18)**						
Constant	-3.06	***	(0.45)	0.16		(0.99)	0.14		(1.23)
Woman	-0.10		(0.10)	0.24		(0.19)	-0.09		(0.22)
Foreign nationality	-0.01		(0.13)	0.53	**	(0.26)	-0.36		(0.41)
Age 16-24 years	0.11		(0.18)	0.30		(0.33)	0.11		(0.47)
Age 25-34 years	0.33	***	(0.11)	-0.06		(0.22)	0.04		(0.34)
Age ≥ 50 years	-0.84	***	(0.15)	-0.14		(0.28)	1.62	***	(0.31)
Low educated	-0.02		(0.12)	0.01		(0.22)	-0.63	**	(0.28)
High educated	0.34	***	(0.11)	-0.80	***	(0.22)	-0.55	**	(0.26)
Cohabiting partner	0.33	**	(0.15)	-0.71	***	(0.26)	-0.35		(0.31)
Working partner	0.15		(0.14)	-0.74	***	(0.25)	-0.52		(0.29)
Having children younger than 18	-0.17		(0.11)	-0.94	***	(0.20)	-1.49	***	(0.27)
Densely populated area	-0.06		(0.10)	0.64	***	(0.19)	0.45	*	(0.24)
Brussels	2.55	**	(0.99)	-3.01		(2.35)	-0.10		(2.62)
Wallonia	1.50	***	(0.56)	-1.59		(1.10)	-1.01		(1.26)
Unemployment rate (province)	-0.22		(2.06)	12.53	***	(4.56)	-0.30		(6.20)
Unemployment rate (region)	-1.08	***	(0.37)	0.42		(0.74)	0.46		(0.87)
% homeowners (province)				0.13		(0.37)	0.17		(0.48)
House price to rent ratio (province)				1.09	***	(0.15)	-1.03	***	(0.27)
Duration dependence									
t = [1] (ref.)									
t = [2]	0.25	*	(0.14)						
t = [3]	-0.11		(0.16)						
t = [4,6]	-0.41	***	(0.14)						
t = [7,9]	-0.88	***	(0.18)						
t = [10,12]	-0.40	**	(0.17)						
t = [13,15]	-0.94	***	(0.28)						
t > 15	-1.68	***	(0.26)						
Unobserved heterogeneity: estimates									
$v_2/u_{1,2}/u_{2,2}$	0.80		(1.31)	-20.00			8.81		(8.19)
q_2				-3.99	***	(0.76)			
Unobserved heterogeneity: resulting probabilities and correlation									
p_1				0.98					
p_2				0.02					
$Corr(v,u_1)$				-1.00					
$Corr(v,u_2)$				1.00					
$Corr(u_1,u_2)$				-1.00					
Log-likelihood				-2645.44					
Akaike Information Criterion				5420.89					
Parameters				65					
N				1013					

***(**)((*)) indicates significance at the 1%(5%)((10%)) significance level. Standard errors in parentheses. Some heterogeneity parameters are estimated as a very large negative or positive number causing a 0 or 1 probability with respect to related housing tenure status for a subset of individuals. This is numerically problematic. When we face this problem, in the spirit of Gaure et al. (2007), we mark the offending parameter as 'infinity', stick it to -20 resp. 20, and keep it out of further estimation.

The outlined results underline the importance of distinguishing outright owners and mortgagees for an adequate analysis of the relationship between housing and labour market outcomes. In particular, they confirm the hypotheses that we derived from Rouwendal and Nijkamp (2010) in the introduction to this paper. Furthermore, they may help to understand the mixed findings in former contributions that did not distinguish the two types of owners. The very high fraction of mortgagees in Denmark (73%) may explain why Munch et al. (2006) find faster exit rates for owners than for tenants. Along the same line of thought, the more balanced composition of the group of owners in the UK, where only 56% are mortgagees, may explain why Battu et al. (2008) find no significant difference in the exit rates of owners and private tenants[17]. In Table 1 we reported data on the composition of the group of owners in Belgium. With 64.6% of them holding a mortgage, Belgium takes a position somewhat in the middle between the UK and Denmark. The empirical results that we present in Table A.4 in the Appendix should then come as no surprise. The table contains the outcome of a more restricted version of our model in which we do not distinguish between both categories of homeowners. Merging outright owners and mortgagees, we find no significantly different exit rate from unemployment compared to tenants anymore. This result is in line with Battu et al. (2008).

Although our methodology does not allow interpreting the coefficients of the other explanatory variables structurally, their sign and level of statistical significance reveal some information about the control variables. The observed effects on unemployment duration on the left side of Table 2, are generally consistent with our expectations. Ceteris paribus, unemployment spells tend to last longer for individuals who are older than 50, not highly educated and not cohabiting. Although the latter is also found by Munch et al. (2006) and Battu et al. (2008), it might seem rather odd. A possible explanation could be the appearance of positive network effects that are associated with having a partner. Also, unemployment replacement rates are slightly lower when cohabiting[18]. Last, we see that regional dummies and the regional unemployment rate help to determine unemployment duration as well.

[17] The percentages that we mention have been derived from the EU-SILC database by Dol and Neuteboom (2009).

[18] Data are available from Van Vliet and Caminada (2012).

The other columns of Table 2 show the results of the simultaneously estimated mixed multinomial logit model for housing tenure. Also for this component of our model, the coefficients of the explanatory variables show the expected sign. We are particularly interested in the performance of the selected instruments. The percentage of homeowners in the province has only low explanatory power. A possible explanation might be the large scale of the province, summing away most variation. In earlier studies, the municipality was selected as the aggregate level allowing for more variation. Much higher predictive power is attained by the provincial relative price of buying a house versus renting in the year of contract/purchase. In line with our expectations, a high ratio causes a higher probability of renting and a lower probability of being an outright owner.

4.2. Additional results and robustness checks

We conducted several sensitivity analyses to test the robustness of our main finding, i.e. the longer unemployment duration for outright owners compared to tenants and mortgagees. Table A.3 in the Appendix shows the estimated coefficients and corresponding standard errors for our main variables of interest. We summarize the results here:

- We re-estimated our basic model first omitting one of the two instruments while maintaining the other. Then we re-estimated the model without including any instruments. The main results of the three additional estimations are shown in columns (2), (3) and (4) of Table A.3. The results without the provincial homeownership rate as instrument are close to the benchmark model. When not including the house price to rent ratio in the year of purchase or contract, the standard errors increase and so does the estimated coefficient for outright owners. The difference between homeowners with a mortgage and tenants is no longer significant. These results are close to those in column (4), the model without instruments. These findings underscore the importance of introducing the innovative relative price of owning versus renting as an instrument.
- We re-estimated our model dropping all individuals older than 50 from our sample. As was clear from our description of the data in Section 2, there is a strong correlation between being older than 50 and being an outright owner. Although we control for age in our estimations, it could be advisable to check whether our results are not in some way driven by this age group. As is well-known, and confirmed in Table 2,

people older than 50 have typically longer unemployment spells. When we drop individuals older than 50, all our basic findings survive. We report the main results of this re-estimation in column (5) of Table A.3.

- Finally, we introduced alternative age variables in column (6). More precisely, instead of four crude age categories, we directly included individuals' age and its square as continuous explanatory variables. All our basic findings again survive.

5. Conclusions

Seminal work by A.J. Oswald (1996, 1997) suggests that homeownership impairs an individual's labour market outcome. A key element is that high costs of buying and selling homes make homeowners less geographically mobile, which reduces the number of suitable vacancies within their reach in the case of job loss. Homeowners should therefore be expected to incur longer unemployment spells than tenants. Existing microeconometric research for the UK and Denmark, however, comes to different conclusions. Battu et al. (2008) find no significant difference in the speed of transition from unemployment into employment among homeowners versus private tenants in the UK. Munch et al. (2006) even observe a faster exit from unemployment into employment among owners than among tenants in a large panel of Danish individuals.

This paper examines the impact of housing tenure choice on unemployment duration in Belgium using EU-SILC micro data for 2003-2008. Our research question and methodology are basically the same as those of the aforementioned studies. We contribute to the literature in distinguishing homeowners with mortgage payments and outright homeowners. We simultaneously estimate unemployment duration by a mixed proportional hazard model, and the probability of being an outright homeowner, a homeowner with mortgage payments or a tenant by a mixed multinomial logit model. To be able to correctly identify the causal influence of different types of housing tenure on unemployment duration, we use instrumental variables. Finding good instruments is always a delicate task. We propose a new (and strong) instrument, which is the relative price of buying versus renting a house at the moment in the past that people signed the contract underlying their current tenure.

Our results show that homeowners with a mortgage exit unemployment first. Outright owners stay unemployed the longest. Tenants take an intermediate position. From the point of view of the Oswald hypothesis these findings cannot be rationalized as the search and transaction costs associated with moving are similar for outright owners and mortgagees. Instead, our results support the theoretical framework developed by Rouwendal and Nijkamp (2010) and the role of housing costs. If the latter are high, liquidity constraints and the induced reduction of consumption generate strong incentives for the unemployed to find a job soon. Search intensity will be high, the unemployment spell short. If housing costs are low, by contrast, search behaviour will be less intense and the unemployment spell longer. The fact that ceteris paribus the monthly burden of housing costs is much higher for mortgagees than for outright owners, with tenants again in the middle (although undoubtedly closer to mortgagees) can rationalize our empirical findings.

Our results also provide a possible explanation for the discrepancy between the former contributions to this literature. When the distinction between both groups of homeowners is not taken into account, the perceived effect of homeownership will basically be the result of the composition of the group of owners. A much higher share of mortgage holders within the group of homeowners in Denmark compared to the UK may explain the different findings of Munch et al. (2006) versus Battu et al. (2008).

Appendix A: Additional tables

Table A.1: Definitions of variables	
Variable name	**Definition**
Tenant	Dummy equals 1 if the household rents the house.
Outright owner	Dummy equals 1 if the household owns the house and no mortgage payments have to be made.
Mortgagee	Dummy equals 1 if the household owns the house and pays off a mortgage.
Woman	Dummy equals 1 for females, 0 for males.
Foreign nationality	Dummy equals 1 if the individual has a foreign nationality, 0 if not.
Age 16-24 years	Dummy equals 1 if the individual is 16-24 years old.
Age 25-34 years	Dummy equals 1 if the individual is 25-34 years old.
Age 35-49 years	Dummy equals 1 if the individual is 35-49 years old.
Age ≥ 50 years	Dummy equals 1 if the individual is ≥ 50 years old.
Low educated	Dummy equals 1 if the individual did not finish secondary education.
Middle educated	Dummy equals 1 in case of a secondary or post-secondary non tertiary degree.
High educated	Dummy equals 1 in case of a tertiary degree.
Cohabiting partner	Dummy equals 1 if the individual lives together with a partner, 0 otherwise.
Working partner	Dummy equals 1 if the individual lives together with a working partner, 0 if not.
Having children younger than 18	Dummy equals 1 if the person has children younger than 18, 0 otherwise.
Densely populated area	Dummy equals 1 if the individual lives in a municipality with a density superior to 100 inhabitants per square kilometer, and either with a total population for the set of at least 50,000 inhabitants or adjacent to a densely-populated area.
Brussels	Dummy equals 1 when living in the region Brussels.
Flanders	Dummy equals 1 when living in the region Flanders.
Wallonia	Dummy equals 1 when living in the region Wallonia.
Unemployment rate (province)	Unemployment rate in the province (continuous number between 0 and 1).
Unemployment rate (region)	Unemployment rate in the region (continuous number between 0 and 1).
% homeowners (province)	Percentage of homeowners in the province of residence.
House price to rent ratio in year of contract (province)	Ratio of the provincial house price index (with Belgium1990=100) to the rent index (with Belgium1990=100), calculated in the year of purchase or contract.

Note: As explained in the text, values are fixed at the start of the unemployment spell for all variables except for the regional unemployment rate.

Table A.2: Model selection (benchmark model)					
	# param.	**Log-likelihood**	**AIC**	**HQIC**	**BIC**
1 type	61	-2654.283	5430.566	6152.887*	5730.726*
2 types	65	-2645.444	5420.888*	6190.575	5740.732
3 types	69	-2642.842	5423.684	6240.7371	5763.211
4 types	73	-2639.318	5424.635	6289.053	5783.844
5 types	77	-2638.714	5431.428	6343.211	5810.320

Note: *: Preferred specification by this criterion.
AIC: Akaike Information Criterion.
HQIC: Hannan-Quinn Information Criterion.
BIC: Bayesian Information Criterion.

Table A.3: Unemployment duration and housing model – Sensitivity Analysis							
Exit to employment	**(0) Benchmark results (Table 2)**	**(1) Estimating the benchmark model with K=1**	**(2) Omitting provincial rate of homeowner-ship as instrument**	**(3) Omitting historical house price to rent ratio as instrument**	**(4) Estimating without instruments**	**(5) Omitting individuals older than 50 from the sample**	**(6) Including age and age² as continuous explanatory variables**
Tenant	-0.24 ** (0.11)	-0.24 ** (0.11)	-0.24 ** (0.11)	-0.22 (0.32)	-0.23 (0.32)	-0.27 ** (0.11)	-0.24 ** (0.11)
Outright owner	-0.50 *** (0.18)	-0.40 *** (0.13)	-0.50 *** (0.18)	-0.81 ** (0.41)	-0.82 ** (0.41)	-0.54 ** (0.23)	-0.48 *** (0.18)
Optimal K	2	-	2	3	3	2	2
Log-likelihood	-2645.44	-2654.28	-2645.56	-2701.89	-2703.20	-2033.56	-2643.29
AIC	5420.89	5430.57	5417.11	5537.78	5536.36	4191.11	5410.57
Parameters	65	61	63	67	65	62	62
N	1013	1013	1013	1013	1013	739	1013

***(**)((*)) indicates significance at the 1%(5%)((10%)) significance level. Standard errors in parentheses. Some heterogeneity parameters are estimated as a very large negative or positive number causing a 0 or 1 probability with respect to related housing tenure status for a subset of individuals. This is numerically problematic. When we face this problem, in the spirit of Gaure et al. (2007), we mark the offending parameter as 'infinity', stick it to -20 resp. 20, and keep it out of further estimation.

Table A.4: Unemployment duration and housing model (restricted) – estimation results						
	Exit to employment			**Tenant**		
Explanatory variables						
Tenant	**-0.05**		**(0.12)**			
Constant	-3.30	***	(0.44)	-0.68		(0.97)
Woman	-0.09		(0.09)	0.26		(0.18)
Foreign nationality	-0.01		(0.13)	0.64	***	(0.25)
Age 16-24 years	0.10		(0.17)	0.34		(0.30)
Age 25-34 years	0.33	***	(0.10)	-0.07		(0.22)
Age ≥ 50 years	-0.99	***	(0.15)	-0.88	***	(0.26)
Low educated	0.01		(0.12)	0.22		(0.21)
High educated	0.36	***	(0.10)	-0.63	***	(0.21)
Cohabiting partner	0.35	**	(0.14)	-0.63	***	(0.24)
Working partner	0.20		(0.13)	-0.64	***	(0.24)
Having children younger than 18	-0.11		(0.10)	-0.57	***	(0.20)
Densely populated area	-0.10		(0.09)	0.54	***	(0.18)
Brussels	2.64	***	(0.92)	-3.02		(2.16)
Wallonia	1.56	***	(0.51)	-1.33		(1.02)
Unemployment rate (province)	-0.55		(2.03)	14.00	***	(4.89)
Unemployment rate (region)	-1.10	***	(0.34)	0.26		(0.70)
% homeowners (province)				0.12		(0.35)
House price to rent ratio (province)				1.44	***	(0.18)
Duration dependence						
t = [1] (ref.)						
t = [2]	0.26	*	(0.13)			
t = [3]	-0.10		(0.16)			
t = [4,6]	-0.41	***	(0.14)			
t = [7,9]	-0.87	***	(0.18)			
t = [10,12]	-0.39	**	(0.17)			
t = [13,15]	-0.95	***	(0.27)			
t > 15	-1.69	***	(0.26)			
Unobserved heterogeneity: estimates						
v_2/u_2	0.66		(0.54)	-20.00		
q_2				-2.64	***	(0.61)
Unobserved heterogeneity: probabilities and correlation						
p_1			0.933			
p_2			0.067			
Corr(v,u)			-1.00			
Log-likelihood			-2348.70			
Akaike Information Criterion			4785.39			
Parameters			44			
N			1013			

***(**)((*)) indicates significance at the 1%(5%)((10%)) significance level. Standard errors in parentheses. Some heterogeneity parameters are estimated as a very large negative or positive number causing a 0 or 1 probability with respect to related housing tenure status for a subset of individuals. This is numerically problematic. When we face this problem, in the spirit of Gaure et al. (2007), we mark the offending parameter as 'infinity', stick it to -20 resp. 20, and keep it out of further estimation.

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